Metaphor, Theories of Concepts and Biological Reductionism

Marina Rakova

Ph.D.
The University of Edinburgh
2001

To the memory of my mother
Svetlana Rakova
who was everything to me

and

to my father
Boris Rakov
without whom not.

Declaration

I hereby declare that this thesis has been composed by myself and is entirely my own work.

Marina Rakova

November 2000

Contents.

PART III.

Abstract

This thesis is an attempt to present a multidisciplinary approach to adjectival polysemy, particularly adjectival polysemy of a metaphorical type, and its underlying conceptual structure. The last ten years have clearly shown a tendency towards reducing the number of meanings and the idea of metaphor as a mechanism of concept formation has been gaining much force, influencing research in linguistics, psychology and cognitive science. Despite that fact, the long-standing tradition of analytic philosophy did not succumb to the attack. However few contentions are shared in these different fields, one is held unquestioningly by almost everyone. It is the literal-metaphorical distinction which is at the heart of both traditional philosophy and the theory of embodied realism.

Drawing extensively on evidence from research on cross-modal perception, synesthesia, double-function terms in cross-cultural studies, child development, psycholinguistic experiments and experiments with brain-damaged subjects, reinterpreting the available data and analyzing in detail theories of concepts contained in cognitive linguistics, lexical semantics and informational semantics, the thesis casts doubt on the validity of the literal-metaphorical distinction, for this class of examples. It stipulates the existence of psychologically primitive concepts, which are likely to be atomic and innate, and offers a no-polysemy view of conceptual structure with implications for linguistic polysemy. It also shows the limits of biological reductionism and emphasizes the need for functional approaches to cognition. The proposed alternative is both unexpected and exciting, and may serve as a basis for bringing together empirical evidence and philosophical coherence in a non-contradictory way.

Typographical conventions.

The following conventions are adopted throughout the thesis:

Double quotes are reserved for words and expressions.

Single quotes are reserved for concepts. Single quotes also replace double quotes in the text of quotations.

Capital letters are reserved for conceptual metaphors. However, as some authors quoted in the thesis do not adopt the same convention, capital letters in quoted text do not always signify conceptual metaphors. The word GESTALT appears in capitals throughout.

Acknowledgements

There are some people without whom this thesis would have never been the same or, perhaps, would have never been at all. I am immensely grateful to:

Professor James Raymond Hurford of the Department of Theoretical and Applied Linguistics, or Jim Hurford as I came to know him, my first supervisor who has patiently born my strivings for the whole period of thesis-writing. Jim has opened up a whole universe of knowledge for me – thanks to him I will never be bored. Without Jim's constant support and encouragement my thesis would have never had the shape it has now. I owe him a lot in intellectual terms. More than I could possibly say here. But you may be sure that whenever you come across something in this thesis that strikes you as very contemporary sounding and involving much empirical knowledge, its ultimate origin may be traced back to Jim's comments and suggestions.

Professor Timothy Williamson, or Tim Williamson, formerly Professor of Logic at the University of Edinburgh, and now Chair of Logic at the University of Oxford, my second supervisor. At one point in time I had no second supervisor and was running around the University departments trying to get somebody to shoulder the burden. I am glad it was Tim who finally agreed to supervise me. Without Tim there would have been a lot less philosophy and a lot more terminological sloppiness in my thesis. And thanks to Tim I manage to avoid here blaming everything that goes wrong on analytic philosophers, which seems to have become a popular move in cognitive sciences recently.

Being a student of either Jim Hurford or Tim Williamson is enough to make one feel proud. And being a student of both of them has worked out surprisingly well. Although, if you imagine receiving supervision from Beethoven and Chopin at the same time, you will understand the feeling I sometimes had when comparing my supervisors' comments on my chapters. However, offered a chance to start from the beginning, I would have preferred it to be the same way.

Doctor Burton Melnick with whom I share my interest in Asch, adjectival polysemy and cognitive linguistics. Although I have tried out my ideas on many people, it is with Burt that I extensively corresponded and received feedback on some sections.

Doctor Galina Makashova, my English teacher, and now a colleague and a very good friend. Without her I would not have started my studies at the time I did. When I came to have classes with her, the only English sentence I could say was "I speak not English". Six months after that I flew to Edinburgh and passed my English test, which allowed me to become a student there. No one but she could have given that to me.

Mr and Mrs Roderick Keay who have been extremely kind in helping me to sort out all the official and financial matters relating to my arrival and stay in Britain. It was nice and reassuring to know that there was somebody whom I could always contact should I need it.

Mrs Antoanetta Redway who arranged my travel to and from Britain so that it was always the smoothest journey, and in whose company I spent many happy days in London.

It seems to be the usual practice to mention our nearest and dearest in the end of the list. Which does not mean that their help and support were not greatly important to us. If anything, it is too easy to underestimate what they are for us and what they have done for us. My heartfelt thanks and my deepest love go to:

My father, Boris Rakov, whose dream it was to see me studying in Britain and who made his dream come true. He also gave me the opportunity to live in a dream world without having to worry about anything but learning something new. Without him in the first place my thesis would have never come to life, which would have been a pity. When someone gives you a new world, as my father did, it is magic, it is a miracle. Thanks to him I have become a different and a better person. Some debts can only be paid with love. And I hope I will not fail his expectations.

My mother, Svetlana Rakova, to whom I always entrusted my happiest impressions and my worst worries. She always knew what I felt at every moment, and could drag me out of any desperation. Her love and support were with me always, and like no one else on earth she could give me the impetus and the will not to give up, to carry on with what I was struggling to do. It takes a good deal of dedication to do that. Without her I would have never done many of the good things I did in my life. I only wish she was with me today.

My partner, Denis Gladkov, who was witness to pangs of depression when things went wrong and fits of glee when I seemed to get them right. He courageously suffered through reading many and many versions of my thesis, and had to spend sometimes long hours trying to persuade me that some passages were not all that bad. He was also my secret source of physics and music examples and analogies scattered in my thesis. I do not know what my thesis would have looked like without him, but it surely would have been worse.

Edinburgh University was a wonderful place to be, and I am very grateful to the staff of the Department of Theoretical and Applied Linguistics – in particular, to Professor Jim Miller and Doctor Ronnie Cann for their exciting lectures and talks, and to Mrs Ethel Jack and Mrs Irene Macleod for their help with many little and not so little things, their excellent company, lots of good will and infallible encouragement I always got from them.

Introduction.

Do you like talking to people who smile at you coldly and give you sharp replies? Or do you prefer people who greet you warmly and are sweet with you? What kind of strong things do you value: strong relationships, strong power, strong tastes, strong attitudes? What kind of soft things do you appreciate: soft pillows, soft colours, soft women, soft voices? Deep people intrigue us and provoke admiration, while deep grief is sometimes too much to bear. Bright light may be blinding, but bright music sets us into a cheerful mood, and bright children with bright eyes are a delight to every parent. Connoisseurs may have a penchant for dry wine, but people with a dry sense of humour often put off their collocutors...

Put together in one paragraph, all these examples appear a bit odd: what do soft pillows have in common with soft voices? And what properties do bright music and bright eyes share? However, any of them used on its own in our everyday speech would not even attract our attention. Nor would it cause comprehension difficulty. When we read in Agatha Christie "A warm sweet feeling of intoxication ran through her veins" or "She spoke with ready enthusiasm, but inwardly she was conscious of a sudden chill" (*Death on the Nile*, p. 20, p. 19, HarperCollins Publishers) we do not pause to calculate the meaning of "warm", or "sweet", or "chill". We do not wonder whether feelings can indeed be "warm" or "sweet", or whether it is possible to be chilly from inside rather than outside.

The kind of people who do pause and wonder about such things are semanticists, philosophers, psychologists and cognitive scientists. The reason one may be impelled to pause is that, on the one hand, the ways which help us to decide whether a pillow is soft or hard are different from the ways which help us to decide whether a voice is soft or harsh. Philosophers call these ways of deciding whether something is A or B (where A and B are incompatible) "truth-conditions": ascertaining that "this pillow is soft" is true is based on different procedures than ascertaining that "this voice is soft". A conclusion that is made frequently is that "soft" plays different roles in "soft pillow" and "soft voice", generally, that there are two different "soft": "soft1" and "soft2", as it were (or more precisely, two different meanings of "soft").

On the other hand, however, our intuition tells us that somehow in our reasoning all the different meanings of "soft" are related, perhaps related more tightly and in a qualitatively different way than the meanings of "cat" and "dog" or even "mother" and "father". For instance, in interpreting novel utterances. If we know what "cat" means, we shall not be able to guess at what "cat and dog" means unless we also know what "dog" means. But if we know what "soft" means, we know what it means both in "soft pillow" and "soft voice". Or

rather, if we know what "soft1" (as in "soft pillow") means, we can guess at what "soft2" (as in "soft voice") means with a good deal of precision. Even when we hear "soft"-phrases which are completely novel for us (as in "soft writing", for example), we do not usually twist our minds in an effort to understand them, we just do understand them (in this case, as writing that is accessible to lay readers) as long as they are relevant to what we have been talking about.

Our intuition is not playing tricks with us: we know that soft pillows and soft voices are different, but we can easily merge their softness in a simile: "Listening to her voice is like laying one's head on a soft pillow". This example is not very apt or poetic, but it is clear. To notice how widespread are comparisons built out of our associations between different dimensions of sensory experience one only has to watch television: "Why wear cotton, if you can wear silk?" could have been about clothes, but it was about Galaxy chocolates, and it worked. Silk is softer and smoother than cotton, tastes also have degrees of softness and smoothness. When we watch this advert, we perhaps want to try a Galaxy, but we do not normally wonder whether it is appropriate to call tastes "soft", as we also call "soft" those objects that give us tactual sensations. The kind of people who wonder about such things also wonder why this is so.

Adjectives such as "soft", "sharp", "clean", "dry", "cold", "deep", "bright", etc. are called polysemous adjectives because they can be used with nouns of different kinds and their meaning is different depending on what nouns they are predicated of. People tend to think of such adjectives as either having a number of different unrelated meanings or as having one major meaning, often called literal meaning, where all its other meanings are considered metaphorical, i.e. derived from the literal meaning and thus related to it (or else, sometimes they think of such adjectives as presently having several literal meanings, but etymologically only one primary literal meaning). So, they say that the literal meaning of "soft" is "lacking hardness or firmness", or "yielding to pressure", or "easily cut", or "having a smooth surface or texture", etc.

In any case, people believe that "soft" is something that can be properly or literally said about clothes or materials (although sometimes they are not being honest with themselves and avoid admitting that meanings cannot be given by definitions, i.e. that "soft" is not really "lacking hardness of firmness" or "having a smooth surface or texture" but something more than that). It follows that when we call voices or tastes "soft" we are not speaking properly in the academic sense of the word. We are speaking metaphorically. Calling voices or tastes "soft" are instances of cross-modal transfers or transfers between sensory modalities. Expressions such as "a soft voice" or "a soft taste" are also sometimes called

synesthetic metaphors. Correspondingly, calling people "soft" (in character, not to the touch) is also metaphorical, and adjectives which can be used both in physical and psychological contexts are sometimes labelled "double-function" adjectives.

Synesthetic and double-function adjectives form the central topic of my thesis. I claim that the way we talk about the literal and the metaphorical, especially with respect to polysemous adjectives, is badly misguided. Consider the following example. In its classical definition, metaphor is a transfer of predicates from one domain where they properly belong to some other domain where they do not belong, but where they can be transferred if there is a certain similarity between what is being transferred and where it is being transferred to. If I call my cat a "dog", it is a metaphor: I know for sure that my cat is a cat, and that an animal cannot be both a cat and a dog. But I can call him a "dog" if he exhibits behaviour typical of dogs and not typical of cats (he wags his tail and carries my bag in his mouth). If I call my cat an "ayatollah", it is also a metaphor: cats do not have religious beliefs as far as we know. But I can call him an "ayatollah" since he is Persian and usually behaves as if he were the spiritual leader of my household.

However, this is not the case with "soft", or "sharp", or "dry". My cat walks softly, and is often soft-tempered but this is not so because he is fluffy or soft to the touch. You remember "on the one hand" and "on the other hand"? In my thesis I show that people who think that the meanings of polysemous adjectives are unrelated are wrong: not only our intuitions, but psychological research as well show that they are interconnected in our thinking. Likewise, I show that people who think that polysemous adjectives have only one primary meaning are wrong because there are no grounds for an explicit comparison and because such an opinion also contradicts the experimental data we have. I claim that so far nobody has given an explanation of different meanings of polysemous adjectives which would render compatible all our intuitions, data and philosophical considerations.

Therefore I begin my thesis with a discussion of cognitive linguistics, the theory which holds that most of our language and cognition is metaphorical. It has recently become very influential and I consider in detail its advantages and drawbacks. I show that the idea of schematic and sensorimotor structuring of our thinking is a valuable one, but that otherwise cognitive linguistics' assumptions are incompatible with empirical data and are philosophically inconsistent. In Part II (chapters 2-5) I discuss evidence from different sources which relates to our understanding of polysemes. Chapter 2 is about the meanings of "hot" and our physiology. It suggests that using the same word for spicy foods and painful sensations is not accidental and follows the same pattern in different languages. Chapter 3 is about cross-modal or synesthetic adjectives. It shows that even at early childhood people are

strikingly similar in their visual-auditory associations and stipulates that the development of human cognition and language might have been a result of an evolutionary change in the mechanisms of sensory integration. Chapters 4 and 5 are about double-function adjectives. There I state a number of findings which show that in historically unrelated languages the same morphemes are used for describing physical and psychological properties; that children do not acquire the psychological meanings of polysemes on the basis of their physical meanings; that psycholinguistic experiments do not uniquely confirm the idea of the primacy of physical meaning; and that experiments with brain-damaged subjects do not support the role of the right hemisphere as metaphoric processor. I conclude that all this should make us reconsider our idea of the literal.

I have explained why the word "metaphor" appears in the title. As to "theories of concepts", in Part III (chapters 6 and 7) I express the view that the right explanation for polysemy has to be sought not in how language is organized, but in how our conceptual structure is organized as long as language conveys our thoughts and we think in concepts. We may occasionally lose the word "cat" (forget how these creatures are called), but most of us rarely lose the concept 'cat' (no longer understand what kind of things are cats). I show that no existing theory of concepts can satisfactorily explain why the meanings of polysemous adjectives are both separable and related. I then offer a theory of no-polysemy and psychologically primitive concepts which should help us move our discussion to a different level. Roughly it amounts to the following: words can be polysemous, but concepts are not. 'Soft' is a psychologically primitive concept in the sense that humans are good at detecting softness in their surroundings independently of what modality the feeling of softness comes from... And as it takes almost two hundred pages to arrive at this view, I will not say here any more about it. If you think that outrageous ideas may prove useful, you should keep on reading. However, a few indications should be helpful to the reader. Thus, the position that I endorse and that I am arguing for includes the following components: conceptual nativism, atomism and the grounding of cognition in perception.

The exposition in my thesis may remind you of a crime fiction story. This is the way I intended it. In a good detective story, there is a problem in the beginning and a number of clues all the way through. The clues seem to be pointing in one particular direction (the literal-metaphorical distinction in my case). However, when they are all considered together, they suggest that we should look for a solution somewhere else. I think that in my solution to the puzzle all bits and pieces fit, although perhaps they do not yet stick closely. To make them stick we need the glue of future research (and this, by the way, is called a "trite" metaphor).

PART 1.

Chapter 1. Theories of the metaphorical basis of cognition.

1. 1. Experientialism and cognitive linguistics on the nature of concepts.

Since the publication in 1980 of Lakoff and Johnson's *Metaphors We Live By* cognitive linguistics has been a very fruitful field of study, offering an entirely new perspective on human language and cognition through reconsidering the notion of metaphor and its role in conceptualization (Lakoff and Johnson 1980, Lakoff 1987, Johnson 1987, Johnson 1988, Turner 1988, Sweetser 1990, Lakoff 1993, Goatly 1997, Lakoff and Johnson 1999). Experientialism (the philosophical core of cognitive linguistics) proved an attractive psychological theory of concepts, its attraction lying in the reduction of a number of concepts to experiential primitives which, via metaphorical projection, become expanded in the course of development to cover the whole area of human knowledge (Edelman 1992, Gibbs 1996, Glenberg 1997, Pinker 1997).

However, the idea of metaphorically derived concepts has also been influentially criticised by other psychologists and linguists who claimed that only those domains whose structure is already known can be restructured metaphorically, and that experientialist postulates assign too much weight to culturally relativistic conceptualizations thereby ignoring restrictions on perceptual processing systems (Dahl 1989, Honeck 1989, Maratsos 1989, Indurkhya 1992, Indurkhya 1994, Murphy 1996, Murphy 1997). Thus, in this chapter I analyze the now dominant cognitive linguistics approach to metaphor which claims that "directly meaningful concepts" plus metaphorical projection should constitute the basic mechanism of cognition and categorization. I show where this approach has gone both right and wrong, and how unwarranted conclusions have been derived from correct observations. Where appropriate I refer to criticisms offered by the aforementioned authors, but much of the argumentation and the way of presenting it are relatively new.

I take it for granted that most people are familiar with what cognitive linguistics is about. Therefore, in this section I would like only to remind the reader of the answer that experientialism or embodied realism (in Lakoff and Johnson's 1999 terminology) provides to the old question of how experience relates to the genesis of concepts. Although much discussed before and outside the experientialist framework, this question took on a new significance with the introduction of metaphor as a bridge from directly meaningful concepts to all other concepts humans possess.

Experientialism is very assertive about the role of the human body in the formation of concepts and, therefore, the notion of "embodiment" is introduced, which means that understanding is being characterized in terms of "our collective biological capacities" (Lakoff 1987: 267) and our having the type of bodies that we do. From this it follows that the meaningfulness of all conceptual structures is immediately tied to the existence of pre-conceptual bodily experiences, i.e., to the operations one performs with one's own body before they become abstracted and explicitly formulated as schemata of action. In short, experientialism holds that there are two types of such experiences, hence structures - basic-level structure, which is defined by GESTALT perception, and kinesthetic image-schematic structure, which includes simple schemata such as CONTAINERS, PATHS, FORCES, etc., and which also determines the conceptualization of orientations and relations such as UP-DOWN or CENTRE-PERIPHERY.

These two structures are rendered directly meaningful and universal because they are directly experienced by everybody in identical ways. In Lakoff's own words:

> "The consideration of certain gross patterns in our experience - our vertical orientation, the nature of our bodies as containers and as wholes with parts, our ability to sense hot and cold, our experience of being empty (hungry) as opposed to filled (satiated), etc. - suggests that our experience is structured kinesthetically in at least a gross way in a variety of experiential domains."(ibid.: 303)

However, with the expansion of human knowledge and the need to express it, new conceptualizations are required. According to experientialism, abstract conceptual structures develop from the two directly meaningful structures via so-called metaphorical projection (in Lakoff and Johnson's 1999 story, "primary metaphors" give rise to complex metaphors which shape abstract concepts). Therefore, abstract conceptual structures are not directly meaningful, but acquire their meaning from their systematic relationship to directly meaningful structures, as long as metaphorical projection can preserve the relations mapped from the physical domains into abstract domains. Metaphor becomes redefined here as a way of "understanding and experiencing one kind of thing in terms of another" (Lakoff and Johnson 1980: 5) or, in an improved definition, "a cross-domain mapping in the conceptual system" (Lakoff 1993: 203). Thus, it loses its old status of a linguistic means of expressing "similar" concepts, and is being postulated as in itself a mechanism of concept formation.

Similar ideas to the effect that directly meaningful concepts become expanded through metaphor are also found in Johnson (1988) and Sweetser (1990). Thus, Johnson speaks of image-schematic structures as the basis for "the nonpropositional dimension of metaphorical processes of inference" (pp. 28-29), where *nonpropositional* means that in such examples as

PURPOSES ARE PHYSICAL GOALS the understanding of purposive acts comes through the experience of physical movement towards a location, and the relation between the two is based on direct correlations in our experience, not on any consciously realized mapping. In the same fashion, Sweetser characterizes linguistic categorization as depending not only on our referential abilities, but also, and most importantly, "on our metaphorical and metonymic structuring of our perceptions of the world" (p. 9). Elaborating on these issues, Pinker (1997) suggested what neurological basis such metaphoric structuring could have:

> "Other primates may not think about stories, inheritances, meetings, and traffic lights, but they do think about rocks, sticks, and burrows. Evolutionary change often works by copying body parts and tinkering with the copy. For example, insects' mouth parts are modified legs. A similar process may have given us our language of thought. Suppose ancestral circuits for reasoning about space and force were copied, the copy's connections to the eyes and muscles were severed, and references to the physical world were bleached out. The circuits could serve as a scaffolding whose slots are filled with symbols for more abstract concerns like states, possessions, ideas, and desires. The circuits would retain their computational abilities, continuing to reckon about entities being in one state at a time, shifting from state to state, and overcoming entities with opposite valence. When the new, abstract domain has a logical structure that mirrors objects in motion - a traffic light has one color at a time but flips between them; contested social interactions are determined by the stronger of two wills - the old circuits can do useful inferential work. They divulge their ancestry as space- and force-simulators by the metaphors they invite, a kind of vestigial cognitive organ." (355-356)

In their latest book Lakoff and Johnson (1999) also take up plausible neurological explanations, but as we shall see below, they do not hold that it is just the "scaffolding" left over from the ancient circuits, but rather that metaphor as a mechanism of concept formation is still very much alive and active in development. If we now put the idea of metaphorical projection aside, what we shall be left with is the assumption that "all cognitive agents sharing certain physiological features inherit the same pre-conceptual reality" (Indurkhya 1994: 67), or otherwise, that the similarity of certain linguistic expressions in different languages may be explained by the similarity of certain physiological features, - both undoubtedly true statements, if not particularly informative. Therefore, in the later sections I will discuss in more detail what significance directly meaningful concepts have for experientialism and cognitive linguistics generally, and how and why metaphorical projection can be eliminated.

In the subsequent discussions I will be using the terms "experientialism" and "cognitive linguistics" almost interchangeably, with that slight difference that I ascribe to experientialism the theory developed in Lakoff and Johnson (1980), Lakoff (1987), Johnson

(1987), Lakoff (1993), Lakoff and Johnson (1999)[1] and apply "cognitive linguistics" to all other research conducted in the framework set by Lakoff and Johnson, even if these researchers did not necessarily share all of their philosophical assumptions. According to Steen (1994), the "cognitive view of metaphor" is not identical with Lakoff and Johnson's views, one of the reasons for this divergence being *experientialism* as a particular philosophical stand, which is considered "immaterial" to the work on metaphor and cognition generally:

> "most cognitive scientists working on metaphor have by-passed these issues and concentrated on the empirical value of the proposal that people live by conceptual metaphors" (p. 9).

However, such an approach to the role of metaphor in conceptualization seems to be somewhat misguided, for it wants to preserve the cognitive value of metaphor without showing where this value comes from, which was originally the asset of experientialism. Therefore, losing its connection to body-mindedness and socio-biological functioning, the idea of metaphor as an invaluable cognitive instrument stops being exploited in any philosophically interesting sense, even though the research on metaphor without embodiment may produce some other good results.

1. 2. On where experientialism has gone both right and wrong.

After Lakoff published his *Women, Fire, and Dangerous Things* (1987), the reactions varied from complete acceptance to full rejection. However, all book reviews seemed to say pretty much the same thing - the issues dealt with were of extreme importance and some suggestions were really worth investigating, but for all that matters, experientialism was not a theory of anything. To get clearer on what is right and what is wrong, in this section I will enumerate the pros and then, in the following sections, move on to the contras, attempting to show where experientialism is a true reductionism and where it misleadingly under- or over-estimates the role of certain parameters of experience. In view of extremely varied reactions to a new status of metaphor, ranging from mostly dignified silence on the part of

[1] Lakoff and Johnson (1999) prefer the expression "theory of embodied realism", but "experientialism" can still be found in their book, particularly as a name for a certain philosophy. Since the content of the theory is not affected, "experientialism" and "embodied realism" are used interchangeably.

philosophers to euphoric fascination[2], particularly on the part of literary theorists, the aim of this analysis is to show which of experientialist ideas are worth taking on and which of them do not hold for reasons of empirical and philosophical inconsistency.

So, I am starting with the pros, which summarize four indisputable achievements of the experientialist framework:

a) The main advantage of experientialism lies in the fact that it *is* reductionism: the variety of concepts is reduced to the primitives of perception and kinesthesia, - therefore human conceptual system and language are rendered comprehensible. Moreover, the idea of universal semantics is very tempting, and it is approached in experientialism through the postulation of pre-conceptual schemata as principles organizing mental representations.

b) One also has to admit that it was very clever of Lakoff to begin with categorization (this is the whole of book I in Lakoff 1987). Having shown that classical theories of categorization cannot predict prototype effects in language among other things, Lakoff, therefore, safeguarded himself from having to take into account logical classes, which are the stronghold of intelligence irreducible to perception. That is, the advantage is in that experientialism is an empirical explanation of many "mental" phenomena.

c) Another advantage of experientialism is that it emphasizes the role of kinesthetic structuring of experience. This way it avoids the need to explain how perception is related to cognition as both are reduced to the application of image-kinesthetic schemata. Thus, schemata operate in perception and transcend sensory modalities (Johnson 1987: 23-25), but they also determine in what categories people can think of their environment (ibid.: ch.5). And what is really attractive in this sort of reductionism is that this idea - the subordination of perceptual activities to kinesthesia - has strong support in Piaget who showed that perceptual structures become gradually submitted to a "general sensory-motor schematization" which is a source of the development of true intelligence.

d) Even if the existence of image-schemata is not immediately detectable in perception, it is evident in linguistic structures. This is where experientialism plays its trump card: linguistic evidence for the bodily structuring of mental representations (cf. the title of the 1987 Johnson's book - *The Body in the Mind*), and for similar conceptual structuring of

---

[2] To give a few examples: "not only language, but our cognition and hence our language, operates metaphorically" (Sweetser 1990: 8) and "metaphor and the mental processes it entails are basic to language and cognition"(Goatly 1997: 1); "metaphor neither pertains merely to phenomena occurring in the domain of poetic language nor is a linguistic phenomenon per se, but rather it is a much more widespread process...; ...recent psychological studies have *fully established* metaphoricality as a central cognitive mechanism"(Shen 1992: 568, italic mine) and "cognitive linguistics provides literary theory with a new, metaphor-centered model of language" (Hart 1995: 2). In short, it is very strongly claimed that: "The historical *error* has been that metaphor belongs to the domain of language. The new era is that metaphor is an important cognitive operation" (Haskell 1987: 2).

different domains (cf. Murphy 1996: 200) is overwhelming, hence the experientialist explanation is the only correct one; unless other, and equally systematic, explanations are found, experientialism wins the ground.

1. 2. 1. Methodological questions.

The methodology accepted here is rather straightforward. Below I consider the pros and show how they can turn out to be the contras and the drawbacks of experientialist philosophy. Often the sections below begin with a critique directed against the older version of experientialism found in Lakoff and Johnson (1980), Lakoff (1987) and Johnson (1987). Where appropriate, I then address the same issues as they are dealt with in Lakoff and Johnson (1999). I believe that sometimes it is more than justified to tackle the questions in such "developmental" perspective: the stress is now on embodiment rather than experience. Much remained the same, but some issues were reconsidered (presumably as a response to influential critical articles mentioned in 1.1, although Lakoff and Johnson (1999) never admit being aware of them) and thus may deserve special attention as they show how Lakoff and Johnson attempt to make experientialism less susceptible to the old critique.

Many of my objections come from "reshaped" Piaget (1961). One may disagree with using Piaget's evidence since later researchers questioned some of his conclusions (see, for instance, Goswami 1992). However, much of recent research still draws productively on Piaget's general theoretical views, methods and classification of stages in development (see, for instance, Bris et al. 1999, Dusek and Eichenbaum 1997, Geert 1998, Matteson et al. 1997, Quartz 1999). Lakoff and Johnson (1999) refer extensively to connectionist modelling to confirm their ideas on conceptual structure; it is interesting to note in this respect that research of "the second generation of cognitive science" uses stages defined by Piaget as a default structure: thus, Shultz et al. (1995) employed Inhelder and Piaget's (1958) operational procedure in the modelling of the development of seriation in children, though in their later work (Mareschal and Shultz 1999) they argued in favour of small continuous changes instead of radical restructuring. Even this, however, shows that it would be unreasonable to discard Piaget's work as outdated. Finally, a similar approach to juxtaposing Piagetian views with more "body"-oriented theories was employed by Carlson (1997) in his commentary on Glenberg's (1997) theory of memory.

One of the main reasons for contrasting here experientialism with Piaget's constructivism is that experientialism strongly connects the emergence of concepts and language to sensory-motor intelligence, just as Piaget does:

> "I believe language to be all of a piece with acquisitions made at the level of sensory-motor intelligence; ... the conditions of language are part of a vaster context, a context prepared by the various stages of sensory-motor intelligence."(1980: 164, 167) and "It becomes clear, therefore, that the source of notions must be looked for in those sensory-motor schemes or in the schemes of actions in general, but not in perception alone even if those schemes do coincidentally contribute to its organisation."(1961: 354)

However, despite the similarity between the two theories in this respect, there are also many differences between them, that makes it possible to use Piaget's evidence to test experientialism. Therefore, I will now briefly discuss three points pertaining to what these differences are, or can be, under certain readings of Piaget.

First of all, Piaget's sensory-motor intelligence is not exactly the same thing as Lakoff and Johnson's kinesthetic-image schemata, because, although for Piaget perception is part and parcel of sensory-motor intelligence, the line dividing the two is clearly visible, while experientialist kinesthetic-image schemata are supposed to define much of perception from the very beginning, and not, like in Piaget, to improve upon perception through the action on objects. Consequently, as we shall see below, it is not always clear from Lakoff and Johnson's writings whether there are any developmental stages in the application of the schemata, and if there are, how the development proceeds. Therefore, when contrasted with Piaget's theory, many of the experientialist assumptions about kinesthetic-image schemata appear insufficiently elaborated, although not disallowing a Piagetian interpretation.

Correspondingly, if Piaget's ideas of the connection between sensory-motor intelligence and language can be shown faulty in some respects, such criticism would work for experientialism too. And it was indeed shown by Chomsky (see in Piaget 1980b: 170-171) that the simultaneity between the appearance of constructions of sensory-motor intelligence and the appearance of language is not logically necessary. Moreover, the evidence from the studies on the rate of acquisition of language in blind children, whose capacity to develop constructions of sensory-motor intelligence is reduced, shows that their linguistic development is not retarded by their blindness, but is, on the contrary, accelerated. Similar observations can be made about paralysed children, demonstrating that various impairments of sensory-motor functions do not generally impede the workings of the language organ.

Second, Piaget refutes empiricism on the grounds that "the system of the operations of representational thought" cannot be derived from the senses because the schematization of action contributes to the content of intelligence, and also because of the contributions made to perceptual processes by "the activities of the subject" (1961: 361, 363). Even a stronger reason for rejecting empiricism is the idea crucial for Piaget that language is a product of intelligence, and not of learning (see 1980a: 57). While experientialism also admits that the

schematization of action is crucial for the development of conceptual systems, it has more empiricist tendencies than Piaget's constructivism does and, as we shall see below, the evidence for how the schematization itself is made possible begs for questions. Moreover, while Piagetian constructivism consists in the constant reconstruction of objects at different stages, where representational intelligence takes over sensory-motor intelligence (cf. the importance of logico-mathematical explanations for Piaget, 1961: 356), this is not the type of constructive processes found in experientialism, where the image-kinesthetic constructions are supposed to follow naturally from encounters with the environment.

Finally, talking about empiricism, constructivism, and nativism, it can be remarked that Piaget's theory is not inconsistent with a nativist interpretation (the one for which I am largely arguing in the thesis), i.e., if the stages of development distinguished by Piaget are understood as stages in the organism's maturation, rather than concept acquisition. Fodor (see in Piatelli-Palmarini 1980: 147) demonstrated that, on logical grounds, Piaget must not be a non-nativist about changes of stage, or a succession of logics, because a more powerful structure (like "having" reversibility) cannot develop from a weaker structure (reversibility being absent before the age of 7-8), if this development is to be considered as learning, i.e., acquisition of completely new information. Thus, every stage should contain the successive stage as its proper part.

According to Piaget himself (see ibid.: 150, 157), he is ready to acknowledge that the previous structure must contain something of the subsequent structure, although only as a possibility, so that a number of possibilities should remain open for a developing organism; or otherwise, that he finds innate functioning, as distinct from an innate "point of departure", sufficient for bringing about possibilities of structures. In this respect, experientialism may seem to be saying practically the same thing, granted that the conceptualizing capacity is innate. However, the experientialist conceptualizing capacity which should lead to inter- and cross-culturally similar metaphorical projections is not sufficiently specific to restrict the number of possibilities (see discussions below), while Piaget's postulation of innate functioning imposes stricter limitations, i.e., that the development proceeds this way, and not another, - a statement that finds confirmation everywhere in Piaget's works.

I would speculate that Piaget rejects that kind of nativism where the succeeding structures should be contained in the previous ones in all totality, regardless of any other factors which may influence the developmental processes, in fact, an anti-developmental kind of nativism. However, nativism does not have to exclude experience, which can shape differently the innately available information:

> "the nativist isn't committed to saying that viruses know about set theory any more than he is committed to saying that viruses have legs; it hardly follows from the fact that viruses don't have legs that legs aren't innately specified... What that implies, it seems to me, is that a theory of the conceptual plasticity of organisms must be a theory of how the environment selects among the innately specified concepts. *It is not a theory of how you acquire concepts, but a theory of how the environment determines which parts of the conceptual mechanism in principle available to you are in fact exploited.* (Fodor 1980: 151)

In this sense of nativism, Piaget's evidence on children's cognitive abilities, and specifically on their conceptual abilities, at different stages of development is perfectly compatible with the nativist idea of innately specified conceptual organizations. And therefore, Piaget's constructivism, as, for instance, redefining of perceptual objects in terms of structures becoming available to the organism with its maturation, can be considered a subtype of nativism or, in a very apt characterization by J. R. Hurford (personal communication) a "dynamic" nativism as opposed to the static nativism of Chomsky.

Thus, one of my purposes here is to show that experientialist findings deserve a very attentive consideration despite the flaws in the theory, and that they may indeed provide new insights into philosophical ideas (see Lakoff and Johnson 1999). But I tend to believe that they should better be given a nativistic interpretation, even at the price of dispensing with the idea of metaphorical projection. There will be much on this topic in what follows.

1. 3. Reductionism or relativism?

Reductionism and relativism are not supposed to go together. However, this is what happens in the case of experientialism. Above, I have outlined why experientialism *can* be considered reductionism. As to relativism, Lakoff (1987) explicitly admits that experientialism is one of the forms of cultural relativism (pp. 334-337). Similarly, Lakoff & Johnson (1980) say that metaphoric mappings are culturally influenced (see, for instance, chapter 11). True, relativism is an extremely elusive notion (as Lakoff observes, there are hundreds of forms of relativism), and however fixed one thinks the structure of human nature to be, there is always the influence of the environment, which can be varied, and which leads to differences in conceptualization and, hence, ways of expression. In other words, there is often room for indeterminacy, and universal constraints on language and experience define the range of possible concepts, without disallowing variation within that range. In this sense, everyone is a relativist to a certain extent.

However, relativism defined with respect to realistic stands on cognition means something more than that; what is more important to its definition is the idea that cultural presuppositions can bear on biologically or computationally fixed parameters to a qualitatively significant extent. Obscuring or even erasing the boundary between biology and culture is also relativism in this sense. Thus, my idea of relativism is different from that of Lakoff, which amounts to the postulation of "alternative ways of conceptualizing things" (Lakoff 1987: 304)[3]. And therefore, my claim is that experientialism is relativism in my sense of the word, or that the supposed universality of directly meaningful concepts and kinesthetic-image schemata is not consistent with the idea of culturally defined conceptualizations.

To outline Lakoff's own views on the issue. What is universal, according to experientialism, is a general conceptualizing capacity, which people share, "regardless of what differences they may have in conceptual systems" (ibid.: 311). Presumably, this conceptualizing capacity consists in the image-kinesthetic pre-conceptual structuring of experience (vertical and horizontal dimensions, balance, inside and outside, etc.), and, on a higher level, metaphorical structuring of abstract concepts on the basis of concrete and directly understood concepts. What are the possible differences in conceptual systems? According to Lakoff, conceptual systems are different if they have different organizations (for example, with respect to spatial location) or if they use different ways of understanding experience (i.e., depending on what concepts are grammaticalized and used spontaneously and what are simply objects of thought). As examples of conceptual systems different from English, he gives the Cora (from the mountainous area of Mexico), for whom basic hill shape is not only conceptualized but has become part of their grammar, and Mixtec, where spatial relations are conceptualized via body-parts, and are also part of the grammar.

According to Lakoff, Mixtec's conceptual organization is different from English conceptual organization with respect to spatial relations on the grounds of systematic polysemy, - the metaphoric mappings from body parts to spatial locations are almost non-existent in English with its system of prepositional relationships, - and systematic conceptualization matters for understanding (ibid.: 317). The position on spatial location concepts is summarized in Lakoff and Johnson (1999) thus:

---

[3] Lakoff also accepts "individualistic relativism", i.e. "a fact that it is possible for an individual to understand the same domain of experience in different and inconsistent ways" (1987: 335). I won't go into this anywhere as I find it difficult to imagine how individualistic relativism could be rendered compatible with the ideas of cognitive rigidity.

> "One way in which languages differ is that, while some have mainly body-centered relations like *in front of*, others have mainly externally-based relations, like *to the north of*, and still others have mixed systems." (1999: 35)

What I find problematic in this discussion of conceptual differences is that it does not allow strict distinctions to be drawn, and that boundaries become blurred. That is, on the one hand, Mixtec and English do have a common denominator, as their conceptualizations of spatial locations are equally tied to bodily experience - English prepositions such as "in" and "out" are also understood and formed through the experience of *bodily* orientations, and thus translation and general understanding are possible. But on the other hand, their respective conceptual systems should be considered different and to a certain extent incommensurable because of the culturally specific understanding of the domain of spatial relations - there is no systematic understanding of locations in terms of body parts in English. The question is: how much does this alleged systematicity matter, if it is essentially the same principle that underlies the two ways of conceptualizing, and if it is possible to imagine that by some historical accident English would have come to have an identical system of conceptualizing spatial locations ("in back of" being complemented by other expressions)?

I would speculate that the emphasis on the two systems being different is due to the general tendency to present the majority of concepts as continuously understood through something else, - in fact, metaphor (see Lakoff 1987: 306). That is, the boundaries between significant and non-significant cultural variations upon image-kinesthetically induced conceptualizations of experience are not stipulated. There is a danger that what this finally will lead to is some sort of Kuhnean incommensurability when we are going to assume that the concept of time before monetary relations is different from the concept of time in the period when monetary relations exist (TIME IS MONEY conceptual metaphor), i.e., conceptual systems become almost entirely dependent on cultural variations. But this is relativism indeed (in the *bête noire* sense of the word), - the state of things when thought becomes reduced to language[4] and universals lose their significance.

Returning now to the Cora example, one can see that it is even more problematic. According to Lakoff,

---

[4] I think here we come across a very serious problem inherent in all cognitive linguistic approaches - an attempt to explain cognition exclusively through language, or even confusing cognition with language. But "theories of linguistic relativity to the contrary, thought is not forever bound by the words in which it must be expressed" (Miller & Johnson-Laird 1976: 291), for otherwise, semantic change, and especially semantic change due to the development of science, would not have been possible. In the long run, what this amounts to is that experientialism cannot provide a theory of reference (cf. Putnam 1975). Similarly, in his review of Lakoff's (1987) book Maratsos (1989), objecting to the idea of conceptual metaphors on the grounds that languages are historical, and that speakers may learn all the meanings separately, remarks that Lakoff himself often calls the speakers' own perceptions of language and cognition "folk myth" (1987: 6).

> "highly structured preconceptual experiences may be different... Cora speakers may have the same conceptualizing *capacity* as we do, but they have a different *system*, which appears to arise from a different kind of fundamental *experience* with space." (1987: 310)

That is, Cora has a conceptual system different from English with respect to the understanding of space because Cora speakers live in the mountains while English speakers do not, and therefore, they experience space differently. However, this is not consistent with some other of Lakoff's own views: the conceptualizing capacity consists in the assimilation of experiences/ concepts into kinesthetic image schemata, which are supposed to be biologically universal as humans have the kind of bodies that they have[5]. Thus, the fundamental pre-conceptual experience should be the same for Cora and English speakers in what concerns their bodily orientations and operations (for example, grasping), regardless of the areas they inhabit. No doubt, there is such a thing as environmental triggering, which is inseparable from the issue of one's survival (if you live in mountains, you'd rather know about living in mountains than living in marsh areas), but this does not mean that the origin of conceptual systems can be reduced solely to environmental factors.

It appears that instead of resolving the reductionism-relativism puzzle, Lakoff and Johnson (1999) make it look even more problematic. Thus, they still hold that conceptual systems vary across cultures, that they are different on the force of their linguistic realization, and that such differences are validated since there is no one correct description of reality (p. 96). However, they deny their theory being extreme relativism, because

> "First, there are directly embodied concepts, such as basic-level concepts, spatial-relations concepts, and event-structure-concepts... Second, primary metaphors make possible the extension of these embodies concepts into abstract theoretical domains." (ibid.)

What is more, spatial-relations concepts are not merely determined by bodily projections, they are supposed to depend on neural structures of the visual system (see p. 40). And thus, we arrive at a contradiction: directly meaningful concepts, and spatial-relations concepts in particular, cannot be relative as they are pre-specified by the neural structures which play the decisive role in their acquisition, but they are relative since different languages have different dominant "frames of reference" (body-biased or not). What Lakoff and Johnson (1999) forget to show is how the visual system can be culturally driven and whether this ever happens to be the case. If, on the other hand, spatial-relations concepts, which do not

[5]The fact that some researchers (McCormac 1986, Fernandez 1991) objected to Lakoff and Johnson's ideas stressing the importance of cultural experiences over primary bodily experiences shows precisely how strong these other views are.

exist in the external world, are "imposed" by our perceptual and conceptual systems (see p. 31), then conceptualizing spatial-relations either in terms of bodies or points of the horizon should come prior to the experience of spatial relations – a view that contradicts the empiricist stands of experientialism (see next section).

So, if we ask now how it is possible, according to experientialism, that different conceptual systems develop from the same pre-conceptual experience, we can receive an answer of two sorts: a) because pre-conceptual experiences can be different, - but this is inconsistent with the idea of embodied concept formation and the grounding of spatial-relations concepts in the visual system; b) because experience from which conceptual structures develop should include more than simply biological endowment, but also social practices and cultural presuppositions, - but then, in the experientialist framework, it is problematic to decide which comes the first. Let us have another look at how the experientialist explanation works: there is overwhelming linguistic evidence that the structuring of concepts is such and such and that it should be universal and pre-conceptual on the force of this evidence. But there is also evidence that this structuring can be and is sometimes different; thus, the reasoning goes, there are kinesthetic image-schemas, basic-level concepts, and primary metaphorical concepts based on universal (*sic!*) experiences that are most likely to be found in all conceptual systems, but still variation in conceptual organization is to be expected (see Lakoff 1987: 336).

However, while this observation of Lakoff's is undoubtedly correct, the question why and on what criteria variations upon conceptualizing universal experiences are to be expected, or to put it in other terms, why there are cultural preferences for some metaphoric mappings and not for others, is left generally unresolved in the experientialist framework. Unless the tension between reductionist and relativist interpretations is overcome, experientialism is bound to remain relativism pure and simple (and this is indeed how it was understood in a number of book reviews of Lakoff (1987)[6], although I hope to have shown that experientialism is very far from being total relativism). The reason for this is that considering biological organization as secondary with respect to cultural organization is what relativism is mostly about.

1. 4. Troubles with empiricism.

One of the most vital objections here is that experientialism suffers rather significantly from a kind of empiricism (note the idea of *pre-conceptual* experience itself) which often undermines the consistency of the idea of directly meaningful concepts. As concerns the nativist-empiricist debate, Lakoff (1987) hesitated between the two, stating that they were "minor variations" of the same view (p. 174). He seemed to accept both nativism, agreeing that at least some concepts can be inborn (see p. 165), and empiricism, assuming that image-kinesthetic structures become acquired from direct experience with the environment as a result of our evolutionary history (see p. 273). Lakoff and Johnson (1999), however, take a full swing towards empiricism, and below is their position that I will be arguing against:

> "When the embodied experiences... are universal, then the corresponding primary metaphors are universally acquired... Universal conceptual metaphors are learned; they are universals that are not innate." (pp. 56-57)

As mentioned above, experientialism assumes continuity between perception and cognition, tying them both to basic pre-conceptual structures of experience (directly meaningful concepts are derived from experience and underlie the whole of the conceptual system). Since concepts are categories, this has its effects on how categorization is understood, and, according to Lakoff (1987: part I), categorizing does not proceed by the formation of classes with necessary and sufficient features as criteria for inclusion or exclusion. On the contrary, categories are mostly formed on the basis of so-called Idealized Cognitive Models (see ibid., p. 68), which use four structuring principles - propositional structure (or frames), image-schematic structure, metaphoric mapping, metonymic mappings, - and which account for prototype effects and radial structure concepts (such as 'mother', - see ibid.: 74-76). For the present discussion, this is significant in the sense that, if the link between perception and cognition is mediated by image-schemata and is, thus, non-arbitrary, embodied, acquired automatically and unconsciously (see ibid.: 154 and Lakoff and Johnson 1999: 18), then perceptual and "intellectual" categories must be in full agreement – the *nihil in intellectu* story told a couple of centuries too late.

However, there are certain problems with this type of category structure. In his review of *Women, Fire, and Dangerous Things*, Garnham (1989), after having admitted that it was good of Lakoff to question natural kinds, writes:

---

[6] See, for instance, Honeck (1989). But I want to emphasize here that the tension between reductionism and relativism is endemic in all universalist schemes, not just Lakoff's, which have a problem in explaining why languages should vary at all.

> "*Women, fire and dangerous things* is a long book and often an unnecessarily prolix one. Many of its claims are vague, and some are dubious, those about the importance of imagery in categorization, for example - remember turn-of-the-century psychology. And the propaganda is often overdone." (p. 417)[7]

Similarly, it was shown by Piaget, that although logical classes are not perceived, they can be represented and manipulated operationally; and that while perceptions are not reversible, operations of intelligence are[8]. Thus, while experientialism is right (for example, in what concerns their discussion of logical relations - see Lakoff 1987: 272-278) in that perception should be "above all the identification of the object as a member of a class" and that the choice of object for perception is "influenced by a conceptual framework" (Piaget 1961: 190), there are two observations to be made with respect to the supposed image-kinesthetic basis of categories. First, empirical perceptual schemata in themselves are not concepts of classes, and the primary assignment of an object to a class occurs following simple perceptual pre-inferences:

> "The anticipation in question remains perceptual of course, and is not a form of a conceptual judgement or mental image, but simply a recognition of the overall form when veridical, or of surprise when not." (ibid.: 191)

That is, perceptual schemata are not concepts of classes pure and simple, because even though objects are delivered to us by our perceptual apparatus (in Piaget's terms "perceptual pre-inferences") as belonging to a certain category (trees, flowers, etc.), mental operations with these categories require more than just recognitional capacities. Having a perceptual concept 'tree' is probably enough for acting in the world, but I doubt that it is enough for classifying objects into trees and not-trees or for being able to transfer structural similarities (e.g., "A human is a tree") as manifested by the processes of analogical reasoning. The fact that humans grow and develop just as trees do is not part of perceptual content.

---

[7] For a good discussion of mental imagery issue see Pylyshyn (1981). For a more recent rejection of the cognitive role of imagery in language see Rousset (2000).

[8] In Piaget's own words: "Consider, for instance, the simplest of the 'groups' of transformations, the one that leads from a state A to a state B, with possible return from B to A. Perception can provide knowledge of states A and B as configurations, or of change of A into B, or of B into A, as movements, that is, as configurations again... But none of these perceptions, nor all of them together, is equivalent to the system constituted by this 'group', however elementary it may be. This is because the group presumes the comprehension of the fact that the change from B to A is only the inverse of the change from A to B, and presumes the subordination of the states A and B to the transformations as such. This system thus constitutes a new totality of a supra-perceptual order, one which is no longer perceptible as a system. That is why, although perception may begin at birth, we have to wait seven or eight years for the achievement of operational reversibility, upon which the elaboration of rational notions is always conditional." (1961: 354-355). Cf. also Putnam (1980) on cross-inductions and imperfections of associationist models of learning.

Second, if the basis for categorization was truly image-schematic in Lakoff's sense, then this would not explain the point made by Piaget concerning the differences between perception and cognition, for, in the experientialist framework, logical classes and relations are conceived as if they were, in a sense, "perceivable" (because the concept of logical relations is a result of the application of image-kinesthetic schemata, - see also the discussions below), and the operational manipulations with them derive from these perceptual grounds. Clearly, then, both perceptions and operations of intelligence become not reversible (and what's more, it remains unclear how systematicity in the operations with indirectly meaningful concepts could have been achieved, if the latter are to be presented as conscious), which takes us back to section 1.3, showing again how such an extreme reductionism imminently leads to relativism.

1. 4. 1. An instance - some problems with the body as a container schema.

Consider now the example of the body as a container, where containment is one of the pre-conceptual kinesthetic-image schemata, and where the body (or, rather, the perception of one's own body) is its source. According to experientialism, the containment schema is derived from experience. The following is a very good quote to show this:

> "an ordinary instance of image-schematic structure emerging from our experience of physical containment. Our encounter with containment and boundedness is one of the most pervasive features of our bodily experience. We are intimately aware of our bodies as three-dimensional containers into which we put certain things (food, water, air) and out of which other things emerge (food and water wastes, air, blood, etc.). From the beginning, we experience constant physical containment in our surroundings (those things that envelop us). We move in and out of rooms, clothes, vehicles, and numerous kinds of bounded spaces. We manipulate objects, placing them in containers (cups, boxes, cans, bags, etc.). In each of these cases there are repeatable spatial and temporal organizations. In other words, there are typical schemata for physical containment." (Johnson 1987: 21)

Lakoff and Johnson (1999) appear to retain the same position on where the container-schema comes from – it is comprehended through the body as we constantly orient our bodies with respect to containers and is then projected onto less clearly outlined areas in space (p. 36). But however exciting the outlined story sounds, it does not seem very plausible and often contradicts existing developmental evidence. To name some of the problems:

Generally, it is not so obvious that children are aware of containment as related to their own bodies; the understanding of this relation appears quite late in development. Thus, for example, to say that we put air into ourselves puzzles me even now. Yes, at very early stages children start experimenting with the capability of some things to contain other things. But, there is something else to be said about it. First of all, and concerning not only this particular example, the permanence of objects is something that appears with age, even if relatively early (see Piaget 1961). Second, air going into humans' lungs is quite an abstract idea to be grasped by children (since what is at stake is not the permanence of an object, but the permanence of matter - some adults cannot understand it), and to allow them to form the mental representation of containment from an instance they are not capable of understanding. And finally, putting something into oneself requires certain activity, however, until the age of 4 or 5 children do not understand themselves as agents (see Wellman 1990: 268)[9].

Johnson speaks of us being intimately aware of our bodies as three-dimensional containers, however, the understanding of dimensions appears rather late in development (for example, the understanding of depth, - the ability to operationally manipulate the abstract representation of 'cube', as opposed to that of 'square', which is not fully established until the age of 9-11). In other words, if the idea of containment were to emerge from our encounter with its instances, this would have never let us detach form from content (see also below on higher intelligence). This consideration makes it even more problematic to explain the projection of container schema acquired from clearly visible physical containers in one's individual experience[10] into "containers" with blurred boundaries, like in "a butterfly in the garden" (Lakoff and Johnson 1999: 31). For one thing, conscious and correct usage of the preposition appears a good while before the reliable understanding of three-dimensionality arises. One might think that the idea of three-dimensional containers is not required to be explicit, but in Lakoff and Johnson's interpretation it is the agents who "have to project a nontrivial amount of imagistic structure onto a scene" (ibid.), and this already presupposes conscious activity.

Similarly, there is a problem with the idea of us being placed into containers, and the emergence of the containment schema from such situations. The understanding of our physical surroundings as containers requires that external reference points be already mastered at that stage (i.e., talking about Johnson's (1987) example, one needs to see both

---

[9] Although Wellman really speaks of children's *theory of mind*, still the idea of agency with respect to what one can do with one's own body is also something that develops.

[10] I emphasize the idea of individual experience as for Lakoff and Johnson conceptual metaphors, and thus their experiential sources, are very much "alive" (Lakoff and Johnson 1999: 86-87)

the room and oneself in it from an outsider's point of view[11]; see also below on decentration). However, as Piaget shows, external frames of reference, as opposed to field effects of primary perception, become correctly estimated only by the age of 9 to 10:

> "This failure [to use the external frame of reference] is not due to a lack of that perceptual activity which allows the child to explore and to relate objects to a system of reference, but rather to a lack of the 'idea' of such possible referrals." (1961: 334)

Perhaps, with respect to the relations described in the passage from Johnson, we should better speak of logical, not physical containment. I have tried to show that, according to the patterns of development, this looks a more natural suggestion. Thus, for example, you can place something into something else only if this second thing is bigger, and only if it is of a suitable shape. So, we are dealing here with size and shape relations, which with respect to the idea of containment can be thought of as an abstract and purely formal relation, not necessarily exploited in all of its possible applications in real experience. Otherwise, what is more relevant with respect to the so-called metaphorical extensions of image-schemata[12], one could speak of the representations of inclusion and exclusion (or else constrainment) as governing this type of abstract relations, the representation that could also serve as an explanatory schema for relations of physical type (one can only be at one place at a time).

The insistence on the existence of kinesthetic image-schemata found everywhere in Lakoff's and Johnson's writings seems somewhat suspicious. Clearly, no one seriously objects to the presence of schematic structures in cognition and imagination. So, it is likely that such insistence is due to the wish to present the schemata as empirical in origin, despite developmental counter-evidence and inconsistency of this claim. Hence, a naturally arising question is why, according to experientialism, image-schematic structures cannot be innate. Johnson (see Johnson 1987 and 1988) likes to refer to Kant while discussing kinesthetic-image schemata, and to compare them with Kant's (*Critique of Pure Reason*) schemata, which the latter introduced to explain how percepts reliably relate to concepts. However, this reference to Kant does not validate the empiricist claim in the least, neither does it validate the idea of continuity between perception and intelligence[13]. In sum, with respect to

---

[11] Cf. "the God's eye view" of objectivist semantics so despised by Lakoff (1987: 261).

[12] Like in Johnson's RACE EVENT AS CONTAINER - "I give up, I'm getting out of the race" (1987: 34).

[13] Not only "Kant's interpretation [of schema] is somewhat limited by his peculiar view of concepts", but he's better interpreted in a experientialist sense. Thus, Johnson quotes from Kant: "The schema of the triangle can exist nowhere but in thought", and then deciphers: "By "thought" here Kant means that schemata are not merely physiological processes but have a reality as structures or patterns of mental representations" (1987: 24). But, alas, Kant did not mean that schemata are the result of our encountering triangles on a regular basis in our everyday experience, and there was no physiological *a priori* for him.

the experientialist explanations of conceptual schematizations, there are more "whys" than "becauses" there, and, in fact, there cannot be any simple progression from perception to intelligence via bodily imagery.

The last point. When Lakoff (1987) says that his "relativistic reductionism" (see 1.3. above) is the only possible solution for the conceptual identity/ diversity puzzle, he gives the following reason:

> "The idea that *people are born with a conceptualizing capacity seems to be the only plausible way* to begin to provide answers for all these questions [like what is the range of possible human conceptual systems?]... The alternative seems to be to assume that all children are born with all concepts that now exist in all cultures, as well as all concepts that have ever existed or will ever exist. On this view, learning a new concept is just the activation of an already existing concept (Fodor, 1975). I find such an idea too bizarre to take seriously." (335, my italics)

What I want to remark with respect to the above quotation is that, while the assumption "that people are born with *a* conceptualizing capacity" is not truly informative, to suggest that there are firm and necessary (in the philosophical sense of the word) correlations between humans' conceptual systems and their environment is not actually such a bad idea, and it is precisely the one to be found in Fodor (1987 and 1998)[14]. True, very often truth-conditional semantics has little to say about cognitive processes underlying the use of language, and this may sometimes lead to the consideration of quasi-problems which would have never arisen if human cognitive agents were taken into account. So, I fully agree with the experientialist critique in this respect. However, not all theories which can be labelled objectivism ignore humans, and Fodor's, as well as other psychologically oriented approaches which take concepts to be causal mind-world relations, is one of them.

While considering both objectivism and experientialism as theories of basic realism (those that are committed to the existence of external world), Lakoff (1987: 158-169) criticizes objectivism on the grounds that there are no fixed properties and relations in the reality itself, but only those projected by humans, which are consistent with the image-kinesthetic structures of cognition. But consider now the idea of containment, which in experientialism becomes a property of pre-conceptual structures of experience, i.e., a purely

---

It may be also interesting to compare this interpretation with Piaget's views on Kantian schemata: "... it is still true that authors like Metzger interpret the most general geometric 'GESTALTs' in an authentically Kantian sense when they claim to discern in them those 'conditions of organisation which are preliminary to all experience'. We have already pointed out... that the idea of perceptual schematization, whose importance the present work has underlined, is subject to a similar inspiration, but in the sense of a genetic and not a transcendental construction." (1961: 363).

human construct. If one comes to think about it, however, it becomes clear that containment is a physical, not a physiological relation, and that its realization in human experience obeys physical, not just physiological laws.

A quote from Lakoff and Johnson (1999) shows that they are having a hard time trying to deny the objective character of containment:

> "A container schema, like any other image schema, is conceptual. Such a container schema can, however, be physically instantiated, either as a concrete object, like a room or a cup, or as bounded region in space, like a basketball court or a football field... It is important to distinguish a purely conceptual schema from a physically instantiated one; they have different properties" (p.32)

It is not clear what "instantiated" is supposed to mean here. Is the fact that my cup can contain tea or coffee a mental construct? Then, experientialist schemata are only a step away from Berkeley's after-images. But if it means that objects in the outside world are capable of holding, say, fluid substances, then containment *is* a relation in the world (which a developing organism picks up according to the above quote from Johnson 1987). And this is what an objectivist may be likely to say: we possess the concept of containment because the world causes us to have it. It is both in the nature of the world and the mind that containment is realized. So, where is the difference?

In sum, while it is undoubtedly true that we can only know the reality as it is made accessible to us through our cognitive abilities, before any general claim about the nature of the mind-world relations can be made, one has to specify, on the basis of available evidence, what exactly our cognitive abilities are, - i.e., from the list of all things we know, to deduce the most economical explanation of why it is possible for us to know it (cf. Chomsky's (1980, 1986) arguments for the innateness of the language organ). Moreover, there are presently no reasons to deny the existence of a fixed link between the "objective" external reality and humans' cognitive system, because humans with all their sophisticated cognitive machinery are a natural part of the external reality as much as anything else.

Especially in an evolutionary perspective, to which Lakoff and Johnson (1999) like to refer (see pp. 18, 91), the human mind need not be postulated as construing reality - the temptation that is admittedly difficult to avoid (see Lakoff 1987: 207) – and the fact that the mind is a product of the "external" reality does not in itself favour empiricism (for an alternative view see, for instance, Plotkin 1997). Whether neuroscience favours empiricism (hence, experientialism) is the topic of the following two sections.

---

[14] Despite the strong empiricist flavour of embodied realism, at one point Lakoff and Johnson (1999) admit that conceptualization, being itself embodied, may come prior to experience (p. 139). But this is a nativist affirmation, even if it is "architectural", and not "conceptual" nativism that is at issue.

1. 4. 2. On embodiment and neuroscience: experientialism and neural modelling.

"Embodiment" can mean different things. It can refer to a complete reduction of mental processes to underlying neural operations, the physical stuff (as in Churchland 1991)[15]. It can refer to the idea that all cognitive operations are grounded in neural transformations and that perhaps the same neural mechanisms and pathways are used for both perception and cognition. Finally, it can refer to the idea that mind is structured by the body and in a body-like manner.

This last idea is characteristic of experientialist philosophy before Lakoff and Johnson (1999). It follows from the assumption that all concepts are ultimately reducible to directly meaningful concepts which arise in individual experience from everyday interactions with the environment through the body. What sort of problems this causes for the empiricist philosophy of experientialism is discussed in the previous sections. Lakoff and Johnson (1999) still hold to this assumption, which is now called "phenomenological embodiment", but which refers to the same idea: the idea that image schemata arise from individual experience of body-schematization and that conceptual systems are shaped by "the commonalities of our bodies" (see pp. 6, 36). However, there is also a second half to the notion of embodiment – it is "neural embodiment" – which is evident, for instance, in colour concepts (the fact that humans have the colour concepts that they do largely depends on the neural circuitry of the visual system) or spatial-relations concepts (those would not be available to us without topographic maps and orientation-sensitive cells).

Lakoff and Johnson mention that neural explanations could also be found for other types of concepts (like those determined by image-schemata), but they do not accept the eliminativist alternative and do not dispense with "cognitive unconscious", which is the functional stratum of cognition and which, in their framework, is metaphor as a mechanism of concept-formation. In this section I will mostly ignore the functional explanations and concentrate on the putative neural explanations intended to justify the experientialist philosophy to see whether the introduction of "neural embodiment" is a real advance on "phenomenological embodiment".

Thus, Lakoff and Johnson (1999) set out to show the embodiment of the mind using artificial neural models. This is, however, the only kind of evidence they use, which is nowhere supported by data from realistic brain studies. The main purpose of incorporating results of neural modelling studies into their latest book seems to be exclusively to

[15] Solving forever the mind-body problem is a frequent endeavour. See, for instance, Humphrey (2000). See also Harnad (2000) why it would not work.

emphasize the absence of the perceptual-conceptual distinction[16], that is to support the idea of metaphorical projection by showing that the conceptual system can be largely reduced to the sensorimotor system, and be developed from it in such a straightforward way. While it is now obvious that many of the neural pathways are used for serving both perceptual and conceptual tasks, some considerations speak against the existence of simple perceptual continuity.

Among them is the fact that paralyzed or blind children experience no difficulty in acquiring language and their cognitive abilities are not impaired as a result of their physical handicap (in particular, their talk about space is not affected; see in Piaget 1980b: 170-171). And although some types of semantic impairments are indicative of motor inferences, and therefore perceptual grounding of conceptual structures (difficulty in comprehending words and pictures of living things with the retained ability to comprehend words and pictures of common functional inanimate objects; difficulty in conceptual processing of certain categories following damage to a particular sensorimotor region – Gainotti et al. 1995), other data from this kind of studies suggest that perceptual and conceptual structures are separated to a significant extent (loss of concrete nouns with the simultaneous retention of abstract nouns, difficulty with body parts, symptoms found in visual agnosia and optic aphasia with intact perceptual abilities but impaired semantic abilities[17]; see, for instance, Warrington and Shallice (1984); see also Caplan (1987: 170-195) for discussion and indication that both imagistic and propositional codes must characterize semantic representations).

Let us consider an example of neural modelling research Lakoff and Johnson (1999) give to show that perceptual mechanisms "can actually do *conceptual* work in language learning and reasoning" (p. 38). Regier's (1996) model, on the basis of simulated retinal input and given spatial configurations together with appropriate linguistic descriptions, learnt not only to label instances correctly, but also to assign novel configurations to their respective categories. Lakoff and Johnson (1999) conclude that the model learnt to accomplish both perceptual and conceptual tasks. But is this indeed so? It is more likely that what was achieved is the ability to perform *perceptual* categorization, which in the brain can be

---

[16] Perceptual-cognitive continuity is precisely what brings experientialism and connectionism so close (on the ideas of continuity in connectionism see, for instance, McClelland, St.John and Taraban 1989).

[17] These symptoms are a controversial issue. Although there is a disconnection of visual information from semantic information, a number of studies show that the availability of both visual and sensorimotor information contributes to better object recognition (Magnie et al. 1999). However, even this is not conclusive as to perceptual-cognitive continuity: concepts for concrete objects can enter into all sorts of semantic relations (e.g., intentional relations), and thus their content cannot be reduced to sensorimotor features.

served automatically and unconsciously by the mechanisms of transduction (cf. Piaget 1961 on perceptual pre-inferences referred to in section 1.4), but not *conceptual* categorization.

For it is doubtful that the model is capable of drawing inferences and entertaining spatial-relations concepts in the absence of any "external" input, i.e., it is doubtful whether its operations are truly conceptual, and whether it has a good likeness to how humans process spatial relations (it is one thing to categorize a relation as "above", but it is a different thing to draw inferences about what this may imply for an object in a particular situation of "above"). And this is where the comparison with Piaget becomes particularly useful. As the reader will recall, in Piaget's theory simple perceptual operations are in the course of development subsumed by operations of intelligence. Perception and cognition are thought of as continuous, but the relationship is that of graded (as opposite to simple) continuity, i.e. there is a hierarchy of perceptual and cognitive mechanisms. Recent neurodevelopmental studies confirm Piaget's constructivism, "supporting the view of human cortical development as a protracted period of hierarchical-representation construction" (Quartz 1999: 48). Thus, cognitive development is not simply a matter of the same neuronal configurations and pathways being employed for novel tasks, but that of hierarchical construction of increasingly complex representations.

If this does not sound convincing, consider another example. Lakoff and Johnson (1999) claim that "the neural structure of motor control must already have all the capacities necessary to characterize aspect... and its logic" (p. 43). Evidently, having brain programs for initiating, continuing and completing actions is a prerequisite for having conceptual structures supporting the logic of aspect, but does this alone amount to having conceptual structures from which an aspect-expressing language could develop? It makes one wonder, in this case, why even higher primates do not come close to conceptualizing aspect-like characteristics (for a good recent review of what animals can and cannot do see Hauser 2000). In short, the neural modelling line of experientialist argumentation which is based on the assumption of simple perceptual-cognitive continuity does not always find support in neuroscience or behavioural psychology. Another aspect of the same problem is discussed in the next section.

1. 4. 3. On embodiment and neuroscience: the richness of structures.

Cognitive linguistics has traditionally approached the question of conceptual structures through analyzing surface linguistic expressions and postulating mechanisms that might give rise to the discovered structuring of semantic fields. It has been empirical linguistic research, but rarely developmentally linguistic. Lakoff and Johnson (1999) change the situation introducing C. Johnson's theory of conflation which is supposed to explain the formation of stable metaphoric associations in childhood and offering putative mechanisms for their neural implementation. Another thing to remember is that in the strong sense of embodiment, we are not just "beings with bodies" any longer, but "neural beings":

> "What we call *concepts* are neural structures that allow us to mentally characterize our categories and reason about them... *An embodied concept is a neural structure that is actually part of, or makes use of, the sensorimotor system of our brains.*" (p. 20)

On the basis of studying the Shem corpus (MacWhinney 1995), Johnson (1997a, b) proposed a theory of conflation to explain the appearance of multiple senses in development. Conflation means two conceptual domains being conflated in experience prior to the use of a conceptual metaphor. The main example of study was the metaphor of knowing as seeing ("I see what you mean"). Johnson concluded that the conceptual metaphor becomes possible following stable associations in early experience of the type "Let's see what in the box", where the two domains are naturally correlated. The two stages in development are postulated: 1) the conflation stage, "during which connections between coactive domains are established and the domains are not experienced as separate", and 2) the differentiation stage, "during which domains that were previously coactive are differentiated into metaphorical sources and targets" (see Lakoff and Johnson 1999: 49).

The theory is supposed to work not only for this particular conceptual metaphor, but for other cases as well. Especially for instances of conflating subjective experiences with sensorimotor experiences, which are claimed to be undifferentiated early in childhood. These are associations which lead later to the stability of such expressions as "a warm smile", "a big problem" and "a close friend". It is suggested that since the experience of affection is regularly correlated in infancy with the feeling of being warm (being held), the association between the two domains becomes automatically "built-in". Later the domains become differentiated, but the cross-domain mapping persists. The results of conflation are called "primary metaphors" following J. Grady's usage (see, for instance, Grady, Taub and Morgan 1996), and they form the basis of all complex metaphors. The last part of the story

(see Narayanan 1997) is that stable cross-domain associations of infancy and early childhood become realized neurally in permanent connections between the networks defining conceptual domains in question, such that the activation of one domain results in the activation of the other domain, and metaphorical thinking becomes "built-in", automatic and unconscious.

I propose to begin with the "functionalist" part of the story and then move to the neural part. Thus, the first question concerns the association of seeing and knowing in early experience. If it were indeed the case that on the basis of statistic prevalence of see-to-know inferences (and granted that most information we receive from vision) children come to acquire an alternative meaning of "see", then the vast majority of languages should exhibit the same conceptual metaphor as the English Seeing is Knowing. However, the consequent does not always hold: Finnish, for instance, does not have the metaphor (Jussi Tuovinen, personal communication, 1999). Does this imply that Finnish mothers are not likely to say: "Let's see what's in the box."? However, what is more important is that seeing does not always entail knowing: not everything a child sees is therefore something he or she can know about (for example, scales, where the function they are designed for is not determined by their overall appearance: it is knowing the function that counts as knowledge, not applying the name to perceptually similar objects). Anyhow, the child will be required to make a *conscious*[18] and socially acceptable inference that if he/she looks, he/she will know (cf. Bloom 2000 that children learn meanings of words not by association, but by guessing at adults' intentionality).

Even if the domain of knowledge may be structured metonymically and conventionally through the domain of seeing, this is hardly the case with adjectives denoting subjective and sensorimotor experiences. This is primarily because Johnson's hypothesis of domains conflation and differentiation contradicts developmental evidence on how children understand polysemous adjectives: they do differentiate at least at the age of 3, rejecting psychological meanings of "physical" adjectives as unacceptable. Later they come to possess the two meanings but consider them independent, whereas the stage of "conflation" or noticing similarities appears reliably only by the age of 10. What's more, the acquisition of psychological meanings was found not to depend on the acquisition of physical meanings (Asch and Nerlove 1960; the whole chapter 4 is devoted to the discussion of their research). Would it be still reasonable to assume that the stipulated period of conflation occurs at the pre-linguistic stage? One might assume so, but one would have a lot of trouble trying to

[18] Conscious not in the sense of actually formulating the conditional, but in the sense of being able to differentiate between the domains in order to make the association. See also what follows below.

close the gap of something like 10 years between the two stages. Apart from that, more mundane considerations make one doubt the acceptability of the theory of conflation. For instance, do only big objects cause big problems in childhood? It would appear that trying to pick up a small object can be a very big problem too[19].

The neural part of story is probably the most interesting one. Lakoff and Johnson (1999) claim that following the conflation of sensorimotor and subjective experiences there appear permanent neural connections in the networks "serving" the two domains, which results in primary metaphors. This way, thinking of problems as "big" or friends as "close" becomes automatic and unconscious in later experience (even though deep at heart we may realize that we are not talking literally). A question that almost immediately comes to mind is why we need to postulate metaphoric connections if subjective experience (the experience of affection, etc.) is present as early as sensorimotor experience. Why can't we just say that the same predicates that are true of sensorimotor experience are true of subjective experience without there being any mediation?

Lakoff and Johnson are prepared to face it. They do not deny that we have "literal" concepts of subjective experience (in fact, that we can be aware of our subjective experience), but they have little "flesh", they do not allow us to make inferences, that is why they borrow inferences from the domain of sensorimotor experience with which they correlate in experience (p. 58). And they can do it precisely because there is simultaneous activation of neural networks for the two domains, which become established in early childhood on the basis of statistically reliable associations. Below are the question and the answer in Lakoff and Johnson's formulation for the MORE IS UP primary metaphor:

> "reasoning about vertical motion in the spatial domain is thus used to reason about quantity. But the reverse is not true. We do not reason about verticality in terms of quantity. If activation flows both ways, why are inferences and language about quantity not mapped onto verticality? Why, for example, does *too much* not mean *too high*?... The theory assumes that a sensorimotor neural system has more inferential connections, and therefore a greater inferential capacity, than a neural system characterizing subjective experience in itself. This is the source of asymmetry of primary conceptual metaphor."[20]

This putative neural explanation is supposed to justify the existence of primary conceptual metaphors, but can it do the job? For all we know, the site for subjective experience is the limbic system (see, for instance, Liotti et al. 2000), the brain's mechanism

---

[19] I've been told that Japanese has an expression that would translate as "close friend" into English. However, the Japanese avoid close physical contact even with children. Other examples will also show that direct associationism is faulty, but the issue at hand is the general principle. See Asch (1958) for why associative explanations do not work for a large number of terms.

for regulating cognitive and emotional processing (Bush et al. 2000). And there are also hypotheses that changes in the limbic system were a prerequisite for the appearance of human cognitive capacities (by which we normally mean conceptualization and language; see Cytowic 1989 or section 3.5.4)[21]. To claim that "a neural system characterizing subjective experience in itself" is grounded in "a sensorimotor neural system", Lakoff and Johnson should have bothered to present at least some evidence showing that in the human brain there are "constitutive" projections from the one to the other, so that, for example, impairments of the latter lead to impaired functioning of the former (and for all we know, this does not happen; for all we know, reduced limbic influence is associated with the dysfunction of the motor system as in Parkinson's disease – see Braak et al. 2000). Connectionist assumptions to the contrary, brains do not support any kind of architecture one may care to impose on them (Cf. Fodor 1997).

One might object that Lakoff and Johnson (1999) are not talking of neural systems as such, but of neural systems as serving our conceptual structures. The objection does not hold, however, as we know even less about those: we do not know how concepts are distributed in the brain, neither to what extent they are grounded in the sensorimotor system (see previous section). But even if we grant that during the conflation period permanent neural connections develop between the domain of sensorimotor experience and the domain of subjective experience, we may still wonder how, in this case, domain differentiation is rendered neurologically possible. If conceptualizing quantity as verticality is a permanent connection established early in development, it follows that quantity qua verticality is the only concept available for mental operations with quantities. And thus one may choose to call it metaphorical, but being necessary it is not metaphorical in any useful sense.

Again, one might object that Lakoff and Johnson do not deny any independent content to the domain of quantity, they only claim that it is inferentially "poor". Neither do they assume that the connection between the domains is necessary ("The neural connections between the domains, which constitute the metaphorical mapping, may or may not be activated", p. 56). But then, the neural story is not much help: the issue discussed is the same that has already been extensively criticized (Maratsos 1989, Indurkhya 1994, Murphy 1996) – only those domains that have sufficient internal structure with relevant inferential

---

[20] As a matter of fact, exercising any measuring activity we do reason about verticality in terms of quantity (centimetres, metres, etc.).

[21] Clancy et al. (2000) offer a theory of developmental conservation across evolution. However, there is also variation in mammalian development which concerns limbic and cortical primate neural events. Given that primates are closest to us on the scale of evolutionary development and that the study of neural events in humans is not always possible for ethical reasons, it may be reasonable to

potential can be metaphorically restructured through some other domain. Indeed, the neural story makes the idea of metaphorical concepts even more problematic: if the connections become epigenetically wired, how then can they be un-wired during the postulated differentiation stage (given that the environment does not change and still supports the associations made during the conflation stage)? So, it follows either that there is no concept of quantity other than quantity-qua-verticality or that primary metaphors are not conceptually necessary. I would opt for the latter.

In the last two sections I have been arguing that putative neural explanations offered by Lakoff and Johnson (1999) in support of metaphorical conceptualization cannot do the job. If anything, they work against the idea of metaphor in the brain. The reason I have been so eloquent on the issue is that it has recently become a fashion to speculate about how the brain works on the basis of anything, but neurophysiological data. Anatomy (as in the idea of embodiment) does not come prior to brain programmes[22]. An analogy may be helpful here: our hands may be a good shape for grasping, but our having hands of this particular shape is hardly a sufficient explanation for why and how we grasp.

1. 5. Experientialism and the mechanisms of perception.

It appears that cognitive linguists lay too much stress on the kinesthetic shaping of experience and conceptualization, forgetting about the initial undifferentiated impact from sensory modalities (as manifested, for example, in cross-modal linkages – see chapter 3) and its effect on cognitive structures. Let us consider how this can affect the experientialist theory as a whole.

Following Piaget again, one could note that leaving primary perception aside, experientialism is quite unable to explain why there should be, for example, perceptual illusions, which are noticeable at the earlier stages of development and disappear later,

---

suppose that it is limbic and cortical neural development that makes humans so special, not sensorimotor development as would follow from what Lakoff and Johnson are saying.

[22] I borrow the term "brain programmes" from Young (1987). Another issue that could have been discussed in this section, but was not for reasons of space limitations is that of representations. Lakoff and Johnson (1999) argue against symbolic representations in favour of "intrinsically meaningful" mental structures (p. 77). "Direct meaningfulness" is for some reason an attractive notion (Edelman 1992, Glenberg 1997; cf. also Millikan's 1984 notion of "Normal" and Dennett's 1995 notion of "real meaning"). However, I'm not sure that serious neurophysiologists will buy it. See Young (1987: chapters 5 and 12) for the view that living physical systems must be representing systems in order to survive and that thinking is computation. (Although Young may appear sometimes to be grounding human capacity to unlimited symbol manipulation in the "representational" properties of DNA, he does not endorse the "selfish gene" programme as does Dawkins 1976).

being submitted to sensory-motor activities. Moreover, the submission of perception to intelligence is itself a stage in development, which is apparent in the move from egocentrism of perception to the decentration of operations (Piaget 1961: 298-299). And talking about egocentrism and decentration, one may ask what perception actually *is* for experientialism. Johnson (1987) says that

> "image schemata operate as organizing structures of our experience and understanding at the level of bodily perception and movement" (p. 20).

Similarly, Lakoff (1987) considers the ability to sense hot and cold as due to the kinesthetic structuring of human experience (p. 303). Thus, perception is understood as the application of kinesthetic-image schemata to incoming information. Although Lakoff and Johnson (1999) provide some broader discussion of perceptual mechanics (ch. 3), with the exception of colour concepts the questions they discuss are again mostly limited to the application of body schemata. This is particularly obvious in the contention that "container schemas, like other image schemas, are cross-modal" (p. 32). An inference that it is possible to draw from this is that kinesthetic schematization operates at all levels of perception. Thus, experientialist schemata become almost equated with perception, and moreover, they are also plans for interacting with objects (Johnson 1987: 21).

While the part concerning operations with objects is undoubtedly true, the reduction of all perceptual information to kinesthetic schematization does not agree with patterns of development. Thus, Piaget showed that perception pure and simple is deforming as opposed to the conserving character of logical relations (p. 306), and that perception, not being schematic itself, can only be improved through schematization. One of the experiments on the perception of equality (pp. 337-343) demonstrated that there are four different stages (4 to 5, 5 to 6, 7 to 9, and 9 to 10 years) of how intelligence influences perception. It also demonstrated that perception gets improved through the establishment of numerous correspondences based on the "subject's actual manipulation of objects" (p. 342), which is an advance in schematization not available at earlier stages. And these are the effects the experientialist explanation of perceptual activities cannot presently account for.

To be fair, one should not neglect the fact that, apart from kinesthetic schematization, experientialism acknowledges a second type of structuring in pre-conceptual experience - basic-level structure as defined by GESTALT perception rather than kinesthetic image-schemata (or motor programs). Experientialists tend to explain the prevalence of basic-level concepts in reasoning through GESTALT perception. It seems, however, that one should not be exactly equating one with the other: basic-level perceptual categories may be delivered by modular visual-input systems as the maximum of information inferred from purely visual

stimuli (Fodor 1983: 94-97), which, nevertheless, does not mean that particular properties of a stimulus are inaccessible for independent perceptual scrutiny (think of how we perceive faces). Presently available developmental evidence also casts doubt on the assumption that conceptual generalizations made at the basic level are defined by the overall perceptual similarity of stimuli (Mandler 1992, Mandler and McDonough 1998) and that children's perceptual processing is driven by GESTALT structures (Younger and Cohen 1985).

And while it is true that elements are not given in perception from the beginning, a very significant fact about perceptual activities is that stimuli reaching different modalities can be analysed into their components, and these components be compared independently of the perception of a stimulus as a whole (one can separate pitch from loudness and hue from brightness or saturation). Thus, due to its lack of interest in primary perception, experientialism cannot account, for example, for synesthetic metaphors which are based on equivalencies between perceptual components from different modalities (see also section 6.2.2). Again, as remarked by Piaget, "one perceives better what can be constructed and reconstructed" (p. 303), and this is what GESTALT theory, with its insistence on the identity of the laws governing both perception and intelligence, cannot explain:

> "there is a qualitative difference between the truly operational structures of intelligence and initial perceptual structures. Both, it is true, are forms of equilibrium and both consist of structures characterised by laws of totality. But the non-additive and irreversible compositions of perceptual structures are succeeded by additive and reversible structures of operations, and it becomes inadmissible to reduce the superior structures to the inferior and to be satisfied with only the one type of structuring defined by the clear but limiting notion of 'GESTALT'" (1961: xxv) [23]

One could object that experientialism redefines the traditional notion of "GESTALT", which no longer means perception of overall shape without detailed internal structure, but that of "an organized, unified whole within our experience and understanding" (Johnson 1987: 44). However, the objection does not hold: if image-schemata become equated with GESTALT structures or irreducible GESTALTs (see ibid.), then it is not clear what the difference between the two is, and perception, just as before, is being understood as determined by image-kinesthetic schemata which are called GESTALT structures. Pre-conceptual GESTALT structure is, for Johnson, evident in our idea of force, whose GESTALT characteristics consist in our being intimately aware of such its properties as

[23] To avoid any misunderstanding. I am not arguing against GESTALT perception as such. It is clear that by and large our visual perception is governed by GESTALT principles (Buhmann et al. 1999, Hess and Field 1999, Spillmann 1997). What I am arguing is that GESTALT perception cannot be concept-constitutive. There is nothing in presently available studies to confirm the philosophical assumptions of GESTALT theory. Hence, the comparison with Piaget.

directionality, interaction, causality, intensity, etc. However, since none of these properties is operationally available at the earliest stages of development (the application of force and the notion of causality to be mastered through experimentation with objects), it is doubtful that force events are perceived with their detailed internal structure. But if this is so, then image-kinesthetic schemata do not require or imply GESTALT perception.

To conclude: the genesis of preconcepts as described in Lakoff (1987) and Johnson (1987) contradicts the psychological evidence found in Piaget and others (cf. Indurkhya 1992). And generally, it is not always obvious to what sort of experience cognitive linguistics ordinarily refers since, speaking ontogenetically[24], kinesthetic image-schemata proposed by experientialism, or sensory-motor operational structures as their equivalents in Piaget's theory, appear relatively late in development being preceded by structures which are closer to immediate perceptual configurations, and to which all thought is at first subordinated. In section 1.2 I stated that one of the biggest advantages of experientialism is the idea of kinesthetic schematic structuring of experience and concepts.

Above I expressed my reservations about the perceptual-cognitive continuity the idea of kinesthetic structuring of experience and concepts may imply, but otherwise I fully subscribe to the view that conceptual organization exhibits schematic structuring, and that much of it may be related to kinesthesia (as evidenced, for instance, by mental rotation and availability of imagistic content for abstract reasoning). The reservations I formulated in this section concern the lack of attention to the role of non-kinesthetic perceptual information in development and adult cognition, and exaggerated importance of GESTALT perception which is not necessary for kinesthetic structuring of experience. These two issues need further elaborating if experientialism is to provide a sound theory of concept formation. To do so, it will have to show how development affects the workings of pre-conceptual schemata. Then the nativist-empiricist debate can be taken up again.

---

[24] I do not deny that experientialism may be a plausible phylogenetic explanation of how the pre-concepts of spatial relations, for instance the container schema, evolved into patterns of mental representations (cf. Pinker's suggestion, section 1.1). However, this does not imply that exactly the same reasoning should necessarily be valid to explain the ontogenesis of concepts, just as knowing that a certain species evolved from some other species does not explain the present morphology or adaptational mechanisms of the former species.

1. 6. Embodiment and general intelligence.

1. 6. 1. Reductionism about abstract concepts: back to relativism again?

The question for this section is whether experientialism can explain general (or in Piaget's terms, "higher") intelligence, and therefore it comes close to the issues of relativism, empiricism, and the origin of concepts. As is probably clear from the above discussions, kinesthetic-image schemata influence not only what is perceived, but also what can be accessible to intelligence as a matter of principle. This postulate follows from the experientialist reliance on directly meaningful concepts from which more abstract concepts are derived via metaphor.

At first the idea was that the relationship between directly meaningful and indirectly meaningful concepts is a simple one, and that the existence of directly meaningful concepts can be explained through the naturalness of certain experiences:

> "Similarly, we would suggest that concepts that are used in metaphorical definitions *to define* other concepts also correspond to natural kinds of experience. Examples are PHYSICAL ORIENTATIONS, OBJECTS, SUBSTANCES, SEEING, JOURNEYS, WAR, MADNESS, FOOD, BUILDINGS, etc. These concepts for natural kinds of experience and objects are structured clearly enough and with enough of the right kind of internal structure to do the job of defining other concepts." (Lakoff and Johnson 1980: 118, my emphasis)

However, soon it became evident that natural kinds of experience cannot be just postulated as such, because it would mean either that one should keep on introducing more and more new kinds of natural experience to the extent of rendering the idea of them uninformative or else that there are too little truly natural and directly understood experiences which are not sufficient to explain the idea of metaphorical structuring of various abstract domains. Therefore, if there are to be directly meaningful concepts, then this is due to their mirroring pre-conceptual schemata, i.e., the concept of journey is directly meaningful because it is a basic level concept which is in complete agreement with the pre-conceptual path schema, that assumes a starting point, movement in a direction, and a final point, to which the movement was directed. It is in this sense of direct meaningfulness as closeness to pre-conceptual structures of experience that abstract concepts can be thought of as metaphorically derivational since they have no "preconceptual structure of their own" (Lakoff 1987: 303).

Thus, in the later view, there appear to be two types of projection - the immediate application of schemata to purely physical experiences and the metaphorical extension of

the same schemata from the physical to the non-physical, i.e., our talk of the non-physical is now considered as twice derivative:

> "The OUT1 [simple moving out of a surroundings] schema, which applies prototypically to spatial orientation, is metaphorically projected onto the cognitive domain where there are processes of choosing, rejecting, separating, differentiating abstract objects, and so forth. Numerous cases, such as *leave out, pick out, take out,* etc., can be either physical bodily actions that involve orientational schemata, or else they can be metaphorically oriented mental actions." (Johnson 1987: 34)

There are some observations to be made with respect to this change of the point of view. First of all, it provokes serious doubt as to whether it is justified to consider kinesthetic-image schemata as derived from experience, since the physical experience itself is supposed to be understood through the schemata (see also above on empiricism). This is not necessarily a problem of circularity, but it has to be shown that those primary experiences that give rise to image-kinesthetic schemata are sufficient for an individual to apply them to all further occurrences in physical experiences that come under the schemata already acquired. Second, there is something puzzling in the idea of naturalness preserved from the earlier talk about direct meaningfulness. That is, as there is nothing in the cases *leave out, pick out, take out*, etc. themselves to show their physical origin, and as they can be equally applied to the descriptions of physical and mental actions, the question is: why does it matter which of the concepts - physical or non-physical - appeared first as long as both of them coexist?

In short, from the computational point of view, what matters is not naturalness but, as it were, plausibility: what kind of operations stand behind these concepts, that allow them to be applied to all sorts of cognitive domains, not whether these concepts have a physical or non-physical origin. In other words, recalling Pinker's suggestion (see 1.1), it is plausible that humans' ability to operate with symbols for more abstract concerns developed from some ancestral circuits with references to the physical world "bleached out", and which thus gave us our "language of thought". However, this does not validate the idea of persistent metaphorical structuring: if something is already in the language of thought, then with respect to this computational ability it is not significant what it is being applied to, i.e., it is a more economical and evolutionary advanced state, if in-out orientation has a higher generality, if there is no need for every new born to acquire the connection between physical and non-physical[25], and if ontogenesis does not have to repeat phylogenesis. Therefore, the

[25] Note that there are cases when the non-physical is understood earlier than physical - simple effects of "conduit metaphor" (words as containers for thoughts – Reddy 1993/1979) may be an easier idea than transitivity of physical containment.

idea of metaphorical cognizing appears to be ontogenetically superfluous (one does not need to acquire two sets of terms).

The latest version of experientialism – the theory of embodied realism – seems to bring the two views together. On the one hand, primary metaphors resemble directly meaningful concepts in that subjective experience – sensorimotor experience associations are made early in development and realized at neural level, hence they can be called natural experiences. On the other hand, complex metaphors (e.g., LIFE IS A JOURNEY) are built out of primary metaphors (PURPOSES ARE DESTINATIONS and ACTIONS ARE MOTIONS) combined with cultural belief (people should have destinations in life) and simple linguistic facts (movement towards a destination is a journey). In this sense, complex metaphors are again twice-derivative. The only real change is that primary metaphors are supposed to be automatic and unconscious through becoming wired at the early stages of experience. However, as we saw in 1.4.3, the putative appeal to neural modelling does not eliminate the possibility to present primary metaphors to consciousness as metaphorical, which makes one doubt as to their indispensability. The criticisms offered against the previous two versions still apply.

One more remark. Conceptual metaphors, which now become reduced to the combination of primary metaphors with cultural beliefs, may prove to be misleading (p. 73). Life may not be a journey with purposes and destinations after all. Presumably, what contributes to this is cultural belief, since primary metaphors are universal *as long as* experiences leading to their formation are universal. But for this very reason primary metaphors are not necessarily universal but also culturally driven. So, this takes us back to the question of relativism, and it follows that conceptual metaphors should be double-checked (on the issue of cultural belief *and* the reliability of primary experience) before we can acknowledge them as "crucial" or "useful" to thought. For all we know about semantics, this can only be done employing non-metaphorical concepts[26].

In short, I would vote with both hands for the intuition that there is "the qualitative feel of sensorimotor experience to abstract concepts" (p. 128), but experientialists are placing it in the wrong perspective. If there are indeed motor inferences for types of action, it is more consistent to suppose that they regulate both travel- and love-related inferences without love-inferences being mediated by travel-inferences, or that the concept of love is not

[26] Or those whose "metaphorical truth" provokes no doubt. The correspondence theory of truth creeps in through the back door after it has been thrown out (pp. 98-102). Whatever it is that makes a situation true according to our understanding of embodied experience (p. 102), it had better correspond to what we may justifiably think of the external world in skeletal literary concepts, otherwise it should not be possible for conceptual metaphors to be false.

inferentially poor[27] (i.e., initiating, continuing and abandoning love-relations is conceptually independent from travel-relations). This is one option for the experientialist to avoid relativism.

1. 6. 2. On embodied logic and the possibility of science.

This section continues the discussion of general intelligence in the context of experientialism. Its purpose is to show that representational intelligence can hardly be reduced to sensorimotor intelligence, and that explaining the possibility of science presents a serious challenge to experientialist assumptions.

In Piaget's story, the operations of intelligence bear upon classes, relations, numbers, and propositions. Thus, to give an account of higher intelligence, experientialism has to show whether image schemata are sufficient to explain logical relations. Such is indeed an attempt made in Johnson (1987), where he assumes that by force of their definite internal structure, image-schemata can be metaphorically extended to constrain inferences and structure humans' understanding of formal relations between concepts and propositions:

> "Metaphorically, we understand the process of reasoning as a form of motion along a path - propositions are the locations (or bounded areas) that we start out from, proceed through, and wind up at... When we actually move from one place to another, we experience ourselves as traversing a path from one bounded area to another. This experience, together with the metaphorical understanding of propositions as locations, provides a basis for our understanding of negation... And, if we understand categories metaphorically as containers (where a thing falls within the container, or it does not), then we have the claim that everything is either P (in the category-container) or not-P (outside the container)." (pp. 38-39)

And if we want to represent vague concepts in a similar fashion, we only have to say that they are not very much unlike those things that were placed into containers but are still sticking out of them. It might have been a bad joke, this, but it shows that one has to be very careful and discriminative when implying that bodily imagery is the basis for understanding logical relations. For example, to derive the law of non-contradiction from the image of containment, one has to have an operational notion of identity - since containers are everywhere in our surroundings, one has to be able to decide what counts as the same and what counts as different. Moreover, there is also a problem of separating form from content, and it is a well-known fact that this ability to separate and form abstract concepts of one and

[27] If Lakoff and Johnson were right, then eating should also be travelling ("half-way through steaks"). But it is doubtful that in an *experientialist* story travelling could be more primary than eating.

the other takes time to develop. And it is hard to see how the ideas of negation or non-contradiction could have developed from experience if they are the ones that operate there, - to know that one has traversed a boundary between two areas presupposes that one already knows that one is no longer in the first area[28].

Suggesting that logic should be understood by focusing on the properties of image schemata, and particularly the bodily experience of containment, Johnson (p. 40) uses as argument the idea that since we are animals it should be only natural that our inferential patterns emerge from our activities at the embodied level. And I think, there is a bit of confusion on Johnson's part between logic as a constraint on knowledge and logic as the process of reasoning. If we are talking about the former, then image schemata are no explanation, since logic in this sense can only be the essence of the image schemata themselves, not an *ad hoc* hypothesis derived from them, and the argument from animals is of no use, as there are constraints too on what sort of information animals process. However, if we are talking about the latter, there is a question as to from what point of view propositions are locations and reasoning itself is a movement along a path.

Because to say that inferential patterns emerge from our activities at the embodied level can only mean that our inferences concern the non-physical, and there are no inferences made about the physical. In this case, it is not clear what "inference" is supposed to mean, and, presumably, this is not what Johnson has in mind. But if it is the way we *speak* about our inferences that should emerge from our bodily experience ("to arrive at a proposition"), the interpretation that agrees with the examples Johnson gives, then it is not clear how this bears on the issue of computing logical relations. Simply because to say that someone has arrived at a proposition A does not in the least clarify how he did arrive there, i.e., the fact that we talk of propositions as if they were locations is no explanation for these computational cognitive mechanisms that make such inferences possible.

So, why does it seem so necessary to experientialism to tie together rational inference and kinesthetic image-schemata? The answer is simple: to allow for metaphorical structuring, which should allow to explain why there is a talk about abstract domains at all. Thus, Johnson (1988) speaks of non-propositional metaphorical inference accompanying pre-conceptual levels of cognition. Similarly, Lakoff (1993) insists that metaphors are not propositional, and metaphoric mappings are not propositions either, but "ontological correspondences" (p. 207; however, this cannot be squared with the view that conceptual metaphors can be misleading – see previous section). What is meant by all this is that

[28] Continuing the comparison with Piaget, it is interesting to note that Piaget thinks of logical structures as mechanisms of an "exact and detailed adaptation to reality" (1980a: 59). Johnson, on the

kinesthetically determined pre-concepts get reflected everywhere in humans' conceptual system via some unconscious mapping process that they chose to call metaphor. That is, as long as an abstract domain can be understood in terms of kinesthetic-image schemata, it is likely to become one of the subject areas humans will be talking about.

Correspondingly, when it comes to scientific knowledge, all of it should be deduced from kinesthesia via metaphors which are believed to be not linguistic shorthand for particular insights but that very knowledge itself:

> "But these are not just cases of "application" of independently existing knowledge. I am suggesting the stronger thesis that such models [fluid-flow and moving-crowd models of electricity] *constitute* an individual's understanding of a phenomenon and thereby influence their acts of inference. The metaphors, or analogies, are not merely convenient economies for expressing our knowledge; rather, they *are* our knowledge and understanding of the particular phenomena in question." (Johnson 1987: 112)

The idea of the role of metaphors and analogies in science proves to be very attractive. Lakoff and Johnson (1999) claim that it is metaphorical thought that makes scientific theorizing possible (p. 128). But they are not alone. It was Hesse (1974) who made the claim loudly and explicitly and it has been popular ever since in some psychological and philosophical literature (see, for instance, Paton et al. 1994). However, there is data showing that scientists disagree: that reasoning by analogy often leads to overgeneralizations. To give a few examples. Spiro et al. (1989) show that inadequate use of analogical reasoning leads to misconceptions in medical students which may be harmful in their profession (among them "focus on surface descriptive aspects with corresponding mistreatment of underlying causation" [p.507], the type of analogy that Johnson claims to be constitutive of scientific reasoning). Baader and Hicks (1992) urge physiologists to discontinue the metaphor of waterfall for vascular dynamics as the analogy between the latter and fluid mechanics is not justified on too many counts.

The lack of scientific knowledge may also lead to badly argued views in psychological theories. The following is a story from an influential analogical reasoning psychologist Dedre Gentner. Gentner (1983) discusses the famous analogy between the solar system and atom, and holds that the analogy maps the following lower-order relations: DISTANCE (x, y), ATTRACTIVE FORCE (x, y), REVOLVES AROUND (y, x), MORE MASSIVE THAN (x, y). Those predicates fall under the higher-order abstraction of central force systems, and this should make the mapping of the lower-order relations comply with this higher-order abstraction (for that reason, such predicates as HOTTER THAN are excluded from the

---

contrary, presents them as a passive assimilation of the environment by an organism.

mapping). However, there are certain problems with this account. The higher-order abstraction of central forces applies indeed to both domains, but it is too general to validate the mapping of the lower-order relations: central forces act in both the solar system and atom, but different physical laws describe the way they act.

Consider jointly the predicates ATTRACTIVE FORCE, DISTANCE, and MORE MASSIVE THAN. In the solar system, attractive force, or gravitation, is determined by mass and the square of distance. No such attraction (but electrostatic) is found in atom. The force acting there is weak interaction, which is not determined by the relation of mass to the square of distance; and the distance from the electron to the nucleus is determined by the electron's energy, not its mass. Moreover, in case of gravitational interactions there can be any distance between the two bodies (depending on the mass), while the electron's orbits are strict and, again, depend on its energy[29]. Presumably, the relation that allowed Rutherford to create his atomic model was REVOLVES AROUND. However, it was shown later that this mapping could be only approximate, since planets have fixed orbits, while electrons' orbits are not orbits in the same sense, but rather the "levels" where electrons revolve. Generally, contemporary physics admits the importance of Rutherford's model, but it was significantly changed by Bohr some twenty or thirty years later after it first appeared. And while it was good for its own time (mid-19th century), it is strange to discuss it as an example of a justifiable analogy in the end of the twentieth century.

This example was intended to show that employing metaphorical or analogical reasoning in scientific discourse cannot be the only means of arriving at scientific theories. Metaphors may play an important role in provoking new insights, but it is literal concepts that are used in formulating theories. And this applies to conceptual metaphors as well. Perhaps Lakoff and Johnson (1999) are aware of this, which explains their inconsistency on the issue of scientific realism. On the one hand, they are not postmodernists about science and believe in scientific results, on the other hand, they deny that science can yield the ultimate understanding of reality (pp. 88-89). In any case, it is hard to see how scientific realism can be compatible with the profoundly metaphoric nature of concepts restricted to body-derived meaningful structures. Hence the inconsistency. On p. 22 Lakoff and Johnson ridicule the idea of the world being carved into categories, but on p. 29 they write that:

---

[29] See, for example, the entries for Rutherford atom and Bohr model in Larousse Dictionary of Science and Technology. I have been told that if Gentner's formula for atomic interactions actually worked, the world would not exist. I would guess that she mixed up gravitational forces with strong interactions existing between protons and neutrons. But the latter are inside-nucleus, not inside-atom interactions. And all these domains are not easily mappable onto one another.

> "[f]or basic-level physical objects..., the link between human categories and divisions of things in the world is quite accurate. We can think of scientific instruments as extending these basic-level abilities to perceive, image, and intervene... Such instruments allow us to greatly extend the range of our categories of mind to fit important distinctions in the world... When our basic-level capacities are extended by scientific instrumentation, our ability to select useful real-world divisions is improved."

But the price to pay for being able to uncover real-world divisions is objectivism: the fact that the real world does not depend on our conceptualizations of it and that the human mind can overcome much of epistemic boundedness imposed by the body. And in order to do science one has to dispose of metaphorical concepts.

Finishing this section, I would like to say that the fallacy of experientialism is that it tends to picture humans as animals with a language, where language is, nevertheless, no matter of semantics, and, thus, it has got very little to do with representational states, and even less with true intelligence which surpasses linguistic limitations. And the rejection of intelligence is something that I find difficult to accept.

1. 7. On where we go from now.

In this chapter I have considered the good and the bad features of experientialism. The criticisms I offered may be insufficient to discard experientialism entirely, but this was not the objective. What I hope it demonstrated is that while starting with true premises, experientialism often arrives at conclusions that do not hold together. Therefore, I should be happy to preserve its good insights, such as reducing as much as possible of human cognition to physiology and bodily structures (Lakoff's "collective biological capacities"). However, I will have to look somewhere else for alternative explanations of how particular reductions are made possible. I am bound to do that because there are tendencies in experientialism that I cannot accept: relativism, empiricism and the implicit denial of higher intelligence.

So, let me clarify some points, which are going to serve as premises for the rest of the thesis. What I am looking for are universal constraints on conceptual structures which do not ignore polysemy but neither lead to culturalistic or individualistic relativism (what happens to experientialism). And thus, I also believe that there is an irreducible qualitative difference between perception and cognition, and when later I speak of the constraints perception imposes on cognition, it will not mean that the two are necessarily governed by the same laws, but only that if two stimuli from different domains are perceived as similar

("perceive" may be a wrong word, though), it is likely that they will be also thought of as, in a sense, equivalent and named accordingly. That is, my interest is similarity has little to do with a conscious effort, but with how things are, or rather how they appear to us. Marks and Bornstein (1987) proposed the following differentiation of types of similarity:

> "Abstract, conceptually based similarities are typically verbal, and often they are conveyed through metaphor. However, many simpler and equally important forms of similarity have roots closer to biological and sensory-perceptual function. Sensory similarities commonly rely on directly given perceptual equivalences rather than on conceptualized or constructed verbal analogies." (pp. 49-50)

I am suggesting that many of the problems experientialism (and not only it) encounters are due to the fact that the wrong discriminations have been made. And the most important of them is the literal-metaphorical distinction equally maintained by objectivists and experientialists. My interest lies in the area of what Lakoff and Johnson (1999) call "cognitive unconscious", but I argued that the idea of metaphorical projection cannot be the correct functional account of human conceptual abilities. Moreover, I think that there are three strong reasons to expel the notion of metaphor from any discussion of cognition:

a) the fact that people use the same words to describe different perceptual experiences can be explained through the similarity effect created by perceptual processing mechanisms, which are at the level below that of pre-concepts. The outputs of perceptual analysis should not be regarded as "species of thought" (Fodor 1983: 42-43)

b) the fact that certain words are used for describing two distinct conceptual domains does not entail that one is metaphorically structured in terms of another ("waste" said about both time and money), since no one knows for sure how concepts are represented. If it can be shown that concept representations are more cognitively efficient and economical when abstracted from their empirical content, then out metaphor goes.

c) the fact that certain conceptual domains seem to be structured on the basis of other domains as evidenced by the way we speak about them (argument as war) does not necessarily demonstrate that metaphor is a cognitive mechanism. If it can be shown that such ways of speaking are not indispensable, and if there are more general cognitive mechanisms, such as inductive reasoning, which explain the ability to bring together two unrelated domains along with other related phenomena, then they should be preferred to metaphorical conceptualization.

In the rest of my thesis I will concentrate on the second option while keeping the other two in mind as well.

PART II.

Chapter 2. Evidence from pain research.

This chapter is built upon the research by David Julius and his colleagues into the nature of capsaicin action on nociceptors (referred to hereafter as Caterina et al. 1997). Its objective is to demonstrate how "hot" can justifiably apply to both heat and spicy foods, and whether the same pattern can be observed and explained for other languages, which do not share with English this metaphor. Admittedly, the following is only a conjecture, and should be considered at that value. Not being an expert on the issues under discussion, I did try to do my best to keep everything clear and probable.

2. 1. Preliminary issues.

First, I have to specify that Caterina et al.'s research had nothing to do with linguistics, and even less with the study of metaphor. Their aim was quite different, and is summarized very generally in the title of this section. It had been known for some time already that the exposure to capsaicin leads to the excitation of sensory neurones and the subsequent perception of pain. So, the supposition behind the research in question was that since capsaicin evokes the sensation of burning pain, the biological target of its action must play an important role in the detection of painful stimuli, and that the identification of "capsaicin receptor" may illuminate fundamental mechanisms of pain production.

Second, some definitions and descriptions. Capsaicin is an excitatory neurotoxin, a natural product of capsicum peppers, or simply, "the molecule that makes chilli peppers seem hot" (Clapham 1997: 783). What is particularly interesting about capsaicin as a noxious chemical stimulus is that it does not mimic the action of known chemical modulators of nociceptor function, such as adenosine triphosphate, serotonin, acetylcholine, bradykinin, substance P, histamine, glutamate, and hypertonic saline (see Caterina et al. 1997: 819). However, its action is similar to that of resiniferatoxin, a capsaicin analogue from the flowering cactus *Euphorbia resinifera* (the action of both of them being blocked by a synthetic antagonist capsazepine).

Finally, the obtained results: using an expression cloning strategy, Julius and colleagues have identified the DNA-encoded amino-acid sequence of the protein that comprises the receptor for capsaicin and painful heat sensation.

2. 2. The capsaicin receptor.

The capsaicin receptor, which is known as vanilloid receptor subtype 1 (VR1), - the site of action of capsaicin in the nociceptive pathway - is a "relatively calcium-selective ion channel with a large single-channel conductance" (Clapham 1997: 783)[30]. That is, capsaicin activates sensory neurones by increasing the permeability of plasma membrane to cations: when the channel is open, the influx of extracellular calcium depolarizes the membrane potential, which leads to the excitation of nociceptors (those sensory neurones which respond differentially to noxious stimuli). What is of particular interest to me here is how the association of "hot" taste sensations with hot temperatures is made physiologically possible, and, therefore, I will reproduce only those passages from Caterina et al. that bear immediately on the issue, leaving aside detailed biochemical or electrophysiological descriptions.

It had been postulated that capsaicin molecules act on a specific receptor within nociceptors (James et al. 1993). To achieve a more detailed understanding of the capsaicin action, Caterina et al. isolated "a cDNA clone that reconstitutes capsaicin responsiveness in non-neuronal cells" (p. 816), and found that the capsaicin receptor is "an integral membrane protein", and seems to be expressed exclusively by small-diameter neurones within sensory ganglia. It was further shown that VR1 responses to capsaicin and resiniferatoxin were within the same order of magnitude as reported for native vanilloid sites in sensory ganglia; and that those responses are blocked by the same concentrations of capsazepine and ruthenium red that inhibit native vanilloid receptors. Finally, it was demonstrated that different subjective ratings of "hotness" of pepper variants (ranging from *Habanero* which has the highest vanilloid content to *Poblano verde* which has almost none) correlate with their rank order potencies as activators of VR1.

Caterina et al. made a suggestion that the highly selective nature of capsaicin action presupposed that "vanilloid receptors serve as specific molecular markers for nociceptive

[30] The receptor is called vanilloid receptor because vanilloid moiety constitutes an essential chemical component of capsaicin and resiniferatoxin structures. According to Liu et al. (1998), its existence was first postulated in 1975 from the "structure-activity analyses of several capsaicin analogues" (p. 569).

neurons" (1997: 821). The suggestion was confirmed by "northern blot" analysis which had shown that a certain species of messenger RNA was largely expressed in trigeminal and dorsal root sensory ganglia (where nociceptors' cell bodies are), but was absent from the spinal cord and brain, even from the spinal cord dorsal horn adjacent to the dorsal root ganglion. It was also shown that, within sensory ganglia, VR1 expression predominates in neurones with small diameters. As they further remark, it was proposed in some other studies that capsaicin receptors' are expressed in the nodose ganglion, where cell bodies of visceral nociceptors are (Holzer 1991), and in the preoptic area of the hypothalamus (Szolcsanyi 1993), which is involved in thermoregulation. However, Caterina et al.'s own assay did not detect VR1 expression at those two locations, implying that there may be different subtypes of vanilloid receptors, as stipulated in Szallasi (1994) and Szallasi and Blumberg (1996).

Another suggestion was that because vanilloid-induced pain has a "burning" quality, "vanilloids and heat may evoke painful responses through a common molecular pathway" (ibid.). Caterina's et al.'s next experiment was designed to test whether the increase in temperature from 22 degrees C to ≈ 45 degrees C (the noxious range) had any effect on VR1 activity. It was found that a large number of cells expressing VR1 "exhibited a pronounced increase in calcium levels within seconds of heat treatment" (ibid.: 822), which indicated that VR1 is activated by noxious, but not innocuous thermal stimuli. The results from other temperature-related experiments showed that, most likely, heat-evoked and vanilloid-evoked responses are mediated by the same entity, which altogether allowed Caterina et al. to suggest that, apart from being a chemical transducer, VR1 is also acting as a thermal transducer *in vivo*, either by itself, or in conjunction with other cellular components. The suggestion received further confirmation when it was found that even in a non-mammalian context VR1's heat sensitivity had the same temperature-response profile as that reported for thermal nociceptors.

Thus, from the above description of Caterina et al.'s work, it is possible to deduce that the similarity of painful sensations produced by hot foods and high temperatures has a strong underlying molecular basis, which explains why "spicy" has "hot" as its equivalent. Similarly, Clapham (1997), who describes Caterina et al.'s article as a "plausible molecular explanation for why we perceive foods that contain capsaicin as hot" (p. 784), suggested that VR1, or the capsaicin receptor, might have been better labelled the HOT channel.

2. 3. Languages and bodies.

The previous section showed how the same pain-detecting mechanism, VR1, is activated by noxious chemical (capsaicin contained in hot peppers) and thermal (hot temperatures) stimuli. We can therefore suggest that "hot" (metaphorical) meaning 'hot' (literal) is a physiologically confirmed fact [31]. Presumably, this meaning of the word had existed in English for a long time. Thus, Kuhn and Reidy's *Middle English Dictionary* traces it back to 1390[32], and gives the meaning of "hõt" as "biting" or "peppery", something that is said of spices (entry 2a).

The OED contains a very good intuitive definition of "hot", which is consistent with the above description: "hot... 5) Producing an effect as of heat or burning, esp. on the nerves of taste or the mucous membrane; pungent, acrid, biting; corrosive; heating, ardent". To give some examples. The first entry is dated 1548: "The Englishemen... dranke hote wynes in the hote wether, and did eate all the hote frutes... that there fell sicke" (Edward Hall, *Chronicle*); 1596: "The Mustard is too hot a little" (William Shakespeare, *The Taming of the Shrew*); 1806: "The dish is... too hot of pepper" (Alexander Hunter, *Culina Famulatrix Medicinae; or Receipts in Modern Cookery*).

A digression. It is possible that the above mentioned meaning of "hot" has its roots in medieval medicine/ pharmacology. Thus, the previous entry for "hot" in the OED says: "4) In the physiology of the Middle Ages, expressing one of the fundamental qualities of humans, elements, planets, and bodies in general", and this meaning of "hot" is dated back to 1000. The most striking example from this section has a late origin, but I find it demonstrative, because it suggests how one meaning of "hot" could have been transformed into the other, and how still another meaning of "hot" in the sense of "furious", "violent", or "passionate" may have been initially related to it, but the connection was lost later: "Pepper... hurtful to hot constitutions" (1599: Henry Buttes, *Dyets Drie Dinner*).

However physiologically justified the use of "hot" to mean 'spicy' is, the problem may be that other languages do not use the same expression, and that "spicy" has other metaphorical connections. Let us consider some examples.

In this respect, Latin comes closest to English, because there the association of hot taste sensations with hot temperatures is as stable as it is in English:

---

[31] However, the ninth edition of COED does not mark "hot" in the sense of pungent *figurative*.
[32] Chaucer: "On this roote [Contrition] eek spryngeth a seed of grace... and this seed is egre and hoot" ("hoot" being another version of "hõt").

hot (of flavours) = *feruidus* (the general term for "hot" being *calidus*)
feruidus = 1) intensely hot, boiling, burning;.. 5) hot to the taste
(*Smith's Smaller English-Latin Dictionary* and *Oxford Latin Dictionary*).

The *Oxford Latin Dictionary* confirms its fifth entry for *feruidus* with a quotation from Plinius Secundus' *Naturalis Historia*: "sapore acri et feruido" (A.D.23-79).

German, Dutch, Russian, and, to a certain extent, Ancient Greek are very different from English, but similar between themselves in that their words for "spicy" have 'sharp' as their first meaning.

German:
hot (to taste) = *scharf* (the general term for "hot" being *heiß*)
scharf = a) sharp (biting, cutting) b) hot, highly seasoned
(*Collins German-English English-German Dictionary*)

Dutch:
hot (hot spices) = *scherpe* kruiden (the general terms for "hot" being *heet, warm*)
scherpe = sharp; hot (spices)
(*Cassell's Dutch Dictionary*)

Russian:
hot = *ostriy, prianiy* (the general term for "hot" being *goriachiy*)
ostriy = sharp; (*ostriy sous* = piquant sauce)
prianiy = spicy
(*Modern English-Russian Dictionary* and *The Oxford Russian Dictionary*)

Ancient Greek:
hot (of taste) = δριμυς (the general term for "hot" being θερμος)
δριμυς = I. piercing, sharp, keen II. said of things which affect the eyes or taste
(*English-Greek Dictionary* and *Greek-English Lexicon*)

French is interesting in the respect that it reserves its equivalent of "hot" to describe hot peppers, but otherwise uses "piquant", as well as Spanish and Italian do[33].

French:
hot = *cuisant* (poivre - 2), *piquant* (moutard - 2), *épicé* (1,2), *fort* (1) (moutard)
(the general term for "hot" is *chaud*)
cuisant = hot (pepper); smarting, burning (pain) (3)
piquant = a) prickly, biting, cutting; b) hot, pungent (1)
épicé = spicy (1); highly spiced, hot (seasoning) (3)
(1.*Collins and Robert French-English English-French Dictionary*;
2.*Harrap's Standard French and English Dictionary*;
3.*Harrap's New Standard French and English Dictionary*)

Spanish:
hot (to taste) = *picante* (the general term for "hot" being *caliente*)
picante = a) hot, peppery, highly seasoned b) *fig.* remark: sharp, stinging, cutting
(*Collins Spanish-English English-Spanish Dictionary*)

Italian:
hot (of food) = *piccante* (the general term for "hot" being *caldo*)
piccante (from *picca*) = pungente, frizzante
(*Sansoni-Harrap Standard Italian and English Dictionary* and
Vocabolario della Lingua Italiana)

---

[33] It is, perhaps, worth noting that the dictionaries I used were somewhat inconsistent in their translations. Thus, Collins and Robert does not translate "hot" as "piquant", but translates "piquant" as "hot" or "pungent". Similarly, Harrap's translates "hot" as "piquant", but does not translate "piquant" as "hot".

One can see that the languages analyzed above do not use exactly the same expression in their designation of hot taste sensations, and that they can be roughly put into two big groups: spicy = 1) hot (English, Latin; French uses the word "burning", which also applies to burning pain, and the painful sensation produced by capsaicin has the "burning" quality (see previous section); cf. also that OED gives "burning" as a synonym for "hot"); = 2) sharp (German, Dutch, Russian, Ancient Greek) or piquant (French, Spanish, Italian).

What is interesting about this second group, is that all the expressions used for describing hot taste sensations are associated with painful sensations of an origin other than temperature sensations, i.e., with painful mechanical sensations, which all have a short, sharp, and intense quality: a) sharp - the most general term which, ideally, includes all of the following ones; b) biting (German, French; cf. also *Middle English Dictionary* and OED, which give "biting" is a synonym for "hot"); c) cutting (German, French and, presumably, Spanish); d) prickly (French); e) stinging (Spanish, although it has now only a figurative meaning); f) piercing (Ancient Greek); and finally, g) piquant (French, Spanish, Italian; other languages, like English or Russian, also use it the same sense)[34].

In sum, in the variety of languages and their expressions, one can notice a pattern that is common to all of them: taste sensations produced by spicy foods are associated with sensations produced by noxious thermal or mechanical stimuli. But is there any evidence showing how this double association can be rendered physiologically possible?

2. 4. Any physiological evidence?

The suggestion that the association of "painful" taste sensations with painful sensations induced by mechanical stimuli may also be grounded in certain physiological properties of the nociceptive pathway, which would explain why many languages use the expressions "sharp" or "piquant" in their designation of spicy food, may be derived from a passage in Caterina et al., where it is assumed that not all responses of VR1 are coherent with the responses to noxious stimuli observed *in vivo*:

> "Our findings... suggest that an *in vivo* role of vanilloid receptors is to detect noxious heat. But not all characteristics of endogenous heat responses resemble those mediated by VR1. In the whole animal, capsaicin pretreatment reduces

[34] "Piquant" came into English through French *piquer*, which means "prick, irritate" (COED). I thought that "piquant" was immediately connected to "pike", but had not found so far any confirmation to this. (But cf. *Le Robert Quotidien*, which defines "piquant" as "Qui présente une or plusieurs pointes acérées capables de piquer, de percer ⇒ pointu", and "pique" as "Arme (d'hast) formée d'une hampe garnie d'un fer pointu").

> responsiveness to noxious thermal stimuli in some, but not all, physiological contexts. Similarly, in cultured sensory neurons, some heat-activated currents are reported to be insensitive to ruthenium red and to exhibit a lower relative contribution from calcium ions. Thus responses to noxious thermal stimuli may be transduced through multiple molecular pathways, only some of which may involve VR1. It is presently unclear whether thermosensitivity is an intrinsic physical property of VR1, but the fact that VR1 confers heat responsiveness to both mammalian cells and frog oocytes indicates that any other requisite component(s) must be widely expressed." (1997: 823)

2. 4. 1. The pain pathway.

In order to check this suggestion, I referred to a recognised authority in pain research - *Pain* by Howard L. Fields (1987). In what follows, I will reproduce the description of the nociceptive pathway as it is found there. Thus, nociceptors, which are primary afferent nerves with peripheral terminals, have two major functions: 1) transduction, or "receptor activation", when one form of energy (either chemical, mechanical, or thermal) is converted into the electrochemical nerve impulse in the primary afferent, i.e., into the form that is accessible to the brain; and 2) transmission, when the coded information is relayed to the central nervous system.

There are three major groups of primary afferents distinguished by the presence or absence of a myelin sheath, the diameters and the conduction velocities of their constituent axons, and, therefore, by their function[35]. These three major groups are: 1) the large diameter Aα fiber myelinated primary afferents; 2) the smaller diameter Aδ fiber myelinated primary afferents; and 3) the small diameter C fiber unmyelinated primary afferents. Aα primary afferents are not differential, and can be excited by mild mechanical stimuli. They do not increase their discharge when more intense stimuli are applied to them, hence, they cannot signal the application of noxious stimuli. Moreover, subjects' reports show that their activity does not produce pain. Thus, they do not play any significant role in nociception. In contrast to them, as Fields writes,

> "a significant number of both small-diameter myelinated fibers (Aδ) and unmyelinated (C) primary afferents can be classified as nociceptors because they respond maximally only when noxious stimuli are applied to their receptive fields" (1987: 18).

[35] "Different functional classes of primary afferents tend to cluster within a restricted diameter and conduction velocity range" (Fields 1987: 16). The evidence that I am reproducing here was obtained from cutaneous nerves, which are the most available for identification and electrophysiological study.

Aδ nociceptors respond to noxious mechanical stimuli, particularly to stimulation with sharp-pointed instruments, and although some of them respond to innocuous mechanical stimuli too, they are not as sensitive to them as Aα mechanoreceptors. Around 20-50% of Aδ nociceptors also respond to noxious heat. Those of them that readily respond to raised temperatures are called mechanothermal nociceptors, the others are called high-threshold mechanoreceptors. An interesting property of high-threshold mechanoreceptors is that, although they do not respond to the first application of a thermal stimulus, they start to respond and give progressively larger responses to repeated applications of thermal stimuli of the noxious range (sensitization). As concerns the responses to chemical stimuli, Fields remarks (p. 22) that although this issue was not systematically studied, certain preliminary studies indicate that a significant number of both mechanothermal nociceptors and high-threshold mechanoreceptors are sensitive to irritant chemicals. Finally, the third major class of primary afferents includes C fiber unmyelinated afferents, which constitute the most common element of peripheral nerves, and the majority of which are nociceptors. The major class of C fiber nociceptors are C-polymodal nociceptors (C-PMN), which respond to all types of noxious stimuli: thermal, mechanical, and chemical, and which are sometimes called very generally "receptors for injury".

The contribution of the three types of nociceptors (the Aδ mechanosensitive nociceptor, the Aδ mechanothermal nociceptor, and the C-PMN) to pain sensation can be summarized as follows: 1) the activation of Aδ fibers by a single stimulus leads to "sharp, intense tingling", the repetitive application of noxious stimuli produces a definitely painful sensation; 2) the activation of C fibers leads to "intense and prolonged painful sensation", even when a single stimulus is applied (the Aδ fiber threshold is lower than that of C fiber, see ibid.:24). Interestingly, these patterns of A and C fibers activation correspond to subjective reports of pain sensation, - the well known phenomena of first and second pain. Thus, for example, LaMotte and Campbell (1978) found that "subjective pain ratings and the discharge in primate C-PMNs were similar functions of temperature over the range of 40-50°C" (Fields 1987: 25). First pain is an "early sharp and relatively brief pricking sensation", and it is elicited by activity in Aδ fibers. Second pain is a "later dull, somewhat prolonged sensation", elicited by activity in C fibers[36]. It was shown (Torebjork and Hallin 1973) that C fiber activity is particularly associated with prolonged burning sensation, and

[36] These observations were confirmed by selective blocking of a) unmyelinated or b) myelinated fibers which lead to a) the disappearance of second pain or b) the disappearance of first pain and the increased intensity of second pain (see Fields 1987: figure 2.7).

Fields (1987) also remarks that when brief thermal stimuli are applied, the second pain sensation often has a burning quality.

2. 4. 2. A suggestion.

Intuitively, it seems that the sensations of first and second pain are exactly those two sensations that are produced by the action of vanilloids on nociceptors: when one bites a chilli pepper, the first sensation is that of an "explosion", which is sharp, short, and intense, while the second sensation is that of a "fire", i.e., the second sensation is prolonged and has a burning quality. So, it might seem that capsaicin should excite both Aδ and C nociceptors. Moreover, if one believes (as I do) that, to a significant extent, linguistic evidence reflects cognitive and/ or, as in this case, perceptual structures, then it should be possible to explain the fact that some languages associate the sensations produced by spicy foods with "sharp" sensations on the same (or similar) physiological grounds as the English usage of "hot" to refer to spicy foods was linked to hot temperature sensations in Caterina et al.'s work. The following quotation from Fields appears to suggest how this could be plausible:

> "These observations demonstrate that both Aδ and C nociceptors contribute to pain sensation. Furthermore, they suggest that the activity of each contributes in a distinct way to the quality of the pain sensation. Despite this it is an oversimplification to identify a given quality of pain sensation with activity in a single primary afferent or even a single class of primary afferents. It is clear that any naturally occurring stimulus to the skin will activate a broad range of receptors. This is most obviously true for mechanical stimuli, which will activate many Aα mechanoreceptors in addition to nociceptors. Noxious heat will activate thermoreceptors, and irritant chemicals cannot be applied without some accompanying mechanical or thermal stimulus" (1987: 25).

What particularly attracts me in this quotation is that in natural processes of nociception, no stimulus activates a single receptor, and this especially concerns chemical stimuli (and capsaicin is one of them). Therefore, my suggestion is that the fact that "hot" (to taste) means 'sharp' (in other languages than English or Latin) also has a physiologically valid explanation.

However aesthetically pleasing this hypothesis may sound, there are certain objections that can be raised against it. Let me mention some of them: a) Fields (1987) notes that different mechanisms may implement the depolarization of nociceptors by thermal and mechanical stimuli:

> "There may be more than one nociceptive transduction mechanism. For example, Aδ mechanical nociceptors are relatively insensitive to heat and chemical stimuli and, when sensitized to heat, show no change in sensitivity to mechanical stimuli. In a survey of a large number of C-PMNs, no correlation was found between mechanical and thermal thresholds. These observations indicate that separate mechanisms underlie transduction of the two types of stimulus" (p. 27; the survey was done by Georgopoulos 1974).

b) Caterina et al.'s (1997) work shows that the capsaicin receptor (the cloned receptor) is "expressed exclusively by small-diameter neurons within sensory ganglia" (p. 817), and that "within dorsal root and trigeminal ganglia, VR1 expression predominates in a subset of neurons with small diameters" (p. 821), i.e., VR1 expression seems to be confined to C fibers. c) To the same effect, Liu et al. (1998) write that painful responses to capsaicin are "a consequence of capsaicin binding to receptors associated with cation-selective channels in the peripheral terminals in polymodal nociceptors" (p. 569). d) Finally, Caterina et al. (2000) demonstrate that mice lacking VR1 "showed normal responses to noxious mechanical stimuli but exhibited no vanilloid-evoked pain behavior" and "were impaired in the detection of painful heat" (p. 306). In sum, it appears that my suggestion cannot be right because Aδ nociceptors, responsible for sharp painful sensations, are not excitable by capsaicin, and because there are no correlations between noxious mechanical and thermal sensitivity.

Nevertheless, I think that the suggestion should not be simply rejected as such, and that there are certain indications that it could be partially valid. Let me give some of such reasons.

a) As Fields (1987) assumes, it is an oversimplification to identify a certain pain sensation with an activity in a single class of primary afferents, i.e., it is only an approximate description to say that the activity in Aδ fibers leads to the sensation of sharp pain, and the activity in C fibers leads to the sensation of burning pain. At the time when Fields was writing his book, as he admits himself, the nociceptive transduction at the receptor level had not yet been studied directly. That is, it is not entirely clear how C-PMNs' responses to thermal, mechanical, and chemical stimuli are correlated between themselves, and the survey conducted by Georgopoulos (1974), indicating the existence of separate mechanisms of transduction, was the only one of this kind.

b) Sann (1998) showed that in the guinea-pig ureter, high-threshold mechanoreceptors (so-called U-2 units with mean conduction velocity in the C-fibre range) are excited by capsaicin and bradykinin. Preliminary studies also indicate that U-2 units can respond to noxious heat (temperature sensations are not normally evoked from the ureter), and, thus,

that they act as polymodal nociceptors similar to those found in the skin and testis[37]. Moreover, Sann refers to some related studies (Longhurst et al. 1984, Hong et al. 1993), which had shown that in other viscera, Aδ fibers can be activated by capsaicin (in Sann's own experiments, only one Aδ fibre was activated by capsaicin, an exception "where a chemical stimulus was more effective than mechanical stimuli" (p. 1307), which confirmed his observations that in the ureter, U-1 units conducting in the Aδ velocity range are primarily mechanoreceptors, while U-2 units can be considered as visceral nociceptors). Therefore, my suggestion receives some confirmation here since Aδ nociceptors in some organs were found to be sensitive to capsaicin, and since those C nociceptors, whose major function is to respond to painful local mechanical stimuli, are sensitive both to heat and capsaicin.

c) The study of vanilloid receptor subtypes is far from completed. Here are some examples to show the existing controversy and indicate how sensations of noxious thermal and mechanical stimuli may be correlated. Since Caterina et al.'s (1997) research it has been widely assumed that it is the same channel that is activated by capsaicin and noxious heat. However, on the basis of their study Nagy and Rang (1999) conclude that "distinct molecular entities, which are both likely to be derived from the VR1 gene product, account for the membrane responses to heat and capsaicin" (p. 10647). Davis et al. (2000) disrupted the mouse VR1 gene, but unlike in Caterina et al.'s (2000) study, in their experiments mice showed normal responses to acute noxious thermal stimuli suggesting that "VR1 is required for inflammatory sensitization to noxious thermal stimuli but also that alternative mechanisms are sufficient for normal sensation of noxious heat" (p. 183). Noxious mechanical stimuli may have similar molecular pathways: "two related [to VR1] cDNAs have been isolated, a stretch inactivating channel with mechanosensitive properties and a vanilloid receptor-like protein that is responsive to high temperatures" (Schumacher et al. 2000: 2756; a receptor for noxious heat structurally related to VR1 is also described in Caterina et al. 1999).

There are also reasons of a more general character which may support my suggestion. Thus, a) there is a problem connected with the application of controlled noxious stimuli *in vivo*, and the application of controlled noxious mechanical stimuli is, probably, the most complicated. b) As Fields (1987) says, neurones of the central nervous system

[37] Although the existence of specific visceral nociceptors is now widely accepted, I believe that the study in question may be relevant for my present purposes. Actually, I think that evidence from visceral nociception is particularly good, because mechanical noxious stimuli play there a bigger role than thermal noxious stimuli, and since visceral C-PMNs *are* excited by capsaicin, then a similar

> "receive convergent input from numerous primary afferents, often of different types, including both nociceptive and non-nociceptive afferents... In addition, the responses of CNS neurons to noxious stimuli are variable because they are subject to inhibitory influences either elicited by peripheral stimulation or originating within the brain itself"(p. 41).

and c) the issue of pain sensation and pain perception may also be relevant here. The point is that there is no one-to-one correspondence between activity in the primary afferent nociceptors and the perceptual experience because, as well as visual experience, etc., pain experience is compounded from the "concurrent inputs of multiple somatic receptors" (see Fields 1987: 25-26). Finally, in a recent article Mezey et al. (2000) confirmed the existence of VR1-expressing neurones in the central nervous system of the rat and human (see 2.2) and concluded that:

> "The finding that VR1 is expressed not only in primary sensory neurons but also in several brain nuclei is of great importance in that it places VRs in a much broader perspective than pain perception. VRs in the brain... may be involved in the control of emotions, learning, and satiety, just to name a few exciting possibilities." (p. 3655).

To summarize. Following Caterina et al.'s (1997) work on the role of vanilloid receptors in nociception, which demonstrated how the linguistic connection existing in English between hot foods and hot temperatures is rendered physiologically plausible, I decided to see if there were other correspondences between linguistics and physiology. My general suggestion was that since all languages I analyzed associate sensations evoked by spicy foods with sensations produced on human nociceptive system by either noxious thermal or noxious mechanical stimuli, the physiological explanation of a small part of the English lexicon must be valid for other lexicons too. I think that the evidence I presented and discussed in this section shows the way in which my hypothesis may be confirmed, although it is up to empirical neural research to show if this is in principle possible.

2. 4. 3. An informal survey.

If, as I assumed above, the fact that "hot" (to taste) means 'sharp' in many languages other than English is physiologically confirmed, then it is natural to suppose that native speakers of English should understand "sharp" (to taste) as more or less synonymous with

---

correlation between mechanosensitivity and chemosensitivity of cutaneous and mucus membranes' nociceptors may exist too.

"hot". I asked informally some people what sense does "This food/dish is too sharp (for me)" make for them[38]. Most answers I received did not support the suggestion, as "sharp" was generally understood as bitter or acidic. However, the two answers I received from native speakers of American English seemed to be exactly what I was looking for. Although they both specified that this sentence was not something that one would say, their replies to "sharp" were: a) pungent, too hot, and b) too spicy.

Further questioning of those speakers who did not consider sharp taste as an equivalent of hot/ spicy taste has shown that they would understand the sentence in question the way I intended it to be understood if it were uttered in an appropriate context by a foreign person. This seems somewhat reassuring: under specific conditions, the sentence may have the intended meaning but, perhaps, due to existent linguistic conventions, it is usually considered "un-English". And thus, I think that the suggestion was not a bad one. Let me give some possible explanations for the diversity of answers I received:

a) first of all, the two speakers of American English (one from Arizona and the other from California) did understand "sharp" as meaning "hot";

b) one of the speakers of British English gave the answer that does not contradict this intended meaning either: sharp (to taste) is "something that hits you, like a shock", - a description compatible with sensations produced by both painful mechanical and thermal stimuli. This same description could also be equally used by somebody else to characterize the experience of hot foods in Mexican restaurants, something like: "There was something that hit me, and it felt like a shock";

c) the fact that native speakers of British English understand "sharp" mostly in the sense of "acidic", while native speakers of American English do not, can probably be explained if "sharp" is an existent culinary term in British, but not American English (rather in a sense of cookery books, than dictionaries). Interestingly, the speakers of British English took some time before they answered the question, while the responses of the speakers of American English were immediate and spontaneous, although I cannot see where the significance of this might lie;

d) it appears that "sharp" (along with other words designating taste sensations) is somewhat ambiguous in English. Thus, COED defines "sharp" as "**5 b** (of food or its flavour) pungent, acid". However, "pungent" and "acid" are not exactly synonymous (one would not say "a pungent lemon"). The same COED definition for "pungent" is "**1** having a

[38] My inquiry was intentionally informal since I was not entirely sure whether the stimulus sentence would produce responses adequate to my purposes. To produce a more formalized enquiry may be a topic for future research. Then, it would also be interesting to find out whether native speakers of

sharp or strong taste or smell, esp. so as to produce a pricking sensation". Correspondingly, its definition for "piquant" is "**1** agreeably pungent, sharp, or appetizing". The OED also gives "pungent" as a synonym of "hot", along with "acrid" and "biting". Finally, pungency seems to be understood as a property of hot peppers. Thus, Caterina et al. (1997) write "capsaicin, the main *pungent* ingredient in 'hot' chilli peppers, elicits a sensation of burning pain..." (p. 816), and Liu et al. (1998) write "capsaicin, the vanilloid responsible for the *pungent* taste of hot peppers, binds to receptors found primarily in polymodal nociceptors" (p. 569, my italics in both quotations). So, if one is to believe the dictionaries, "sharp" can, and does, mean "hot" in British English. How, then, are the answers of the British English speakers to be interpreted? Presumably, the association of "sharp" with "acidic" was stronger for them (and the more "conventional" the speakers are, the less eager are they to accept a "non-standard" meaning of "sharp"), although the association of "sharp" with "pungent" or "hot" is still (physiologically) possible for them.

e) Finally, extending these observations to linguistic evolution, one can suppose that initially the whole physiologically defined range of expressions for describing taste sensations is available to a language, and the selection of a particular expression depends on whether some of these expressions were or were not already used for describing a totally different sensation. Thus, for example, Russian has two words corresponding to the English "sharp", one of them is used to describe hot foods ("ostriy"), and the other is used to describe acidic foods ("reskiy")[39]. It might well be that the alternative of "sharp" for spicy taste sensations did not develop in English because "sharp" had been already established to mean "acidic". Anyway, one has to keep in mind that all of the above suggestions are only speculations, but speculations with a strong physiological basis.

In sum, in this chapter I considered a story about why "spicy" is "hot" in some languages and "sharp" in others. This was the first step towards the idea that certain linguistic expressions that may seem metaphorical on the strength of conventionality in languages, should not be considered proper metaphors, because they have their origin not in the analogical or metaphorical projection from one domain to another, but in certain objective facts about our physiology or the external world. Therefore, this class of metaphors has nothing to do with cognition whatsoever, since they are not metaphors after all. The next step – a discussion of synesthetic expressions – is even more significant, for it will turn out that linguistic/ conceptual structures are not quite reducible to physiology.

---

those languages where "sharp" means "spicy" are able to understand sentences with the meaning "This food/dish is too hot (for me)".

Chapter 3. Evidence from synesthesia.

3. 1. Why synesthesia?

According to COED, synesthesia is "the production of a mental sense-impression relating to one sense by the stimulation of another sense". Or, in other words, synesthesia is a psychological property characterised by the perception of a stimulus from one sensory modality as simultaneously accompanied by perceptual experiences proper to some other modality (as in the famous phenomenon of colour-hearing). People for whom such experiences are persistent, stable and usually lifelong are called synesthetic individuals, and they form only a small portion of the population, although a number of researches show that most people are capable of cross-modal associations similar to those that occur in synesthetic perception.

In the context of present discussion synesthesia interests me because of the so-called synesthetic metaphors ("bright sound" being a very simple example), and because that type of metaphor, although bearing immediately on the question of how perception relates to language, for some reason was not extensively explored in cognitive linguistics (I return to this in chapter 6). In this respect, it is interesting to note that some time ago there was a strong tendency to explain synesthetic perception through semantic mediation, i.e., to make it relative to and derivative from linguistic dimensions. This idea was explicitly present in Osgood, Suci and Tannenbaum (1957) where the authors claimed that synesthetic imagery is intimately tied with language metaphor and that both are semantic relations (p. 23).

However, there is evidence to believe such claims erroneous, as Cytowic (1989a) demonstrated that true synesthetic perception and its imitation in non-synesthetic individuals are not exactly comparable. Thus, one may have to distinguish between synesthesia, which is relatively rare, and simple cross-modal associations, which can be made by practically anyone. Therefore, in this chapter I would like to contrast Richard Cytowic's study of synesthetic perception with experiments conducted by Lawrence Marks and colleagues on whether non-synesthetic individuals are able to produce perceptual associations akin to those of synesthetic individuals.

In his earlier works (1974, 1975, 1978, 1982a, 1982b), Marks considered synesthesia as a manifestation of connotative meaning in a sensory form, but in the later works that I am going to discuss here (Marks et al. 1987 and Marks and Bornstein 1987) the emphasis

[39] Interestingly, in other contexts, there will still be a difference between the terms used. Thus, "sharp knife" is "ostriy" in Russian, while "sharp sound" is "reskiy"; using these expressions the other way

shifted onto the idea of verbal processes gaining access to perceptual knowledge, and onto synesthesia being only a very strong manifestation of some universal principles of perception. The purpose behind the comparison of the two studies is to see whether there are any interesting similarities between them, and, more importantly, what the relation between perception (including synesthetic perception) and cross-domain linguistic comparisons could be. As the authors mentioned provide no definitive answers to these questions, the suggestions I offer in this chapter have to be considered as theoretical speculations, although, in the long run, accessible to empirical confirmation or disconfirmation. Finally, at the end of the chapter, I will propose some considerations as to how a theory of concepts could deal with the questions raised by synesthesia and cross-modal associations.

3. 2. Marks on synesthesia.

As mentioned in the previous section, in his earlier experiments Marks was interested in how cross-modality matches influence people's understanding of synesthetic metaphors:

> "The ways that people evaluate synesthetic metaphors emulate the characteristics of synesthetic perception, thereby suggesting that synesthesia in perception and synesthesia in language both may emanate from the same source - from a phenomenological similarity in the makeup of sensory experience of different modalities." (1982a: 177)

One assumption behind the experiments was that there is a psychologically real similarity between such sensory qualities as brightness and loudness, the idea supported by the fact that psychophysics recognizes certain attributes of perception as common to all sensory modalities (in this case, intensity). And it is exactly in terms of similarity across perceptual domains that Marks and colleagues discuss synesthesia. Thus, Marks and Bornstein (1987) consider similarity as a primary psychological phenomenon serving its purposes in a variety of cognitive processes. They address themselves to the most difficult kind of similarity - cross-modal similarity where physically different stimuli which act on different sensory systems nonetheless produce sensations that are perceived and treated as similar.

Another assumption, clear from the quotation above, is that language and/ or the conceptual system somehow reflect psychological similarities, thus contributing to the almost univocal interpretation of synesthetic metaphors, even though the relationship between the two is not that of one-to-one mapping: thus, for instance, "bright sound" does

---

round will be comprehensible, though somewhat funny.

not simply mean 'loud'. Marks et al. (1987) suggest that cross-modal metaphors are closely connected to cross-modal perceptual similarities; Marks and Bornstein (1987) relate the two through the notion of "similarity-as-discovered", which is the origin of "similarity-as-created" (see p. 63). That is, cross-modal similarity is a psychological primitive (and thus discovered, though not in the outside world but in phenomenological experience), while the range of expressions it gives rise to is much broader and cannot be reduced to bare sensory impressions (and in this sense similarities can be created)[40].

With similar assumptions in mind, Marks et al. (1987) conducted a series of experiments with the goal of determining how children aged from 3.5 to 13.5 perceive auditory-visual cross-modal relations. Specifically, it was supposed that cross-modal metaphors are closely connected with synesthetic perception, and that they may show ways in which language incorporates perceptual knowledge. Moreover, it was thought that the understanding of cross-modal metaphors by children may serve as a general model of metaphor comprehension. The experiments in question were patterned after the earlier experiments with adults reported in Marks (1982b), and used three sets of measures: 1) perceptual matches between auditory and visual stimuli; 2) ratings of perceptual stimuli with respect to their intensity - brightness and loudness; and 3) ratings of verbal expressions describing auditory and visual perceptions with respect to their literal and metaphorical meanings, where the stimuli used were adjectives, nouns, literal adjective plus noun compounds and synesthetic metaphors. The purpose of the experiments was to test the suggestion that the rare phenomenon of synesthetic perception relies on a common core of cross-modal similarities manifested in people's understanding of cross-modal metaphors, and that the origins of cross-modal associations can be traced to childhood. The summary of the results of Marks et al.'s experiments follows below.

The first experiment compared pitch with brightness, and it was found that from an early age children understand the cross-modal perceptual equivalencies between high-pitched and bright, and low-pitched and dim (the similar experiment with adults has shown this to be the strongest equivalence). As to the difference between literal and metaphoric meanings of verbal stimuli, it was established that the literal distinctions tend to be larger than the metaphoric ones (the difference between "sunlight" and "moonlight" being larger with respect to brightness than with respect to pitch). It was also clear that, to a great extent, the metaphoric interpretations followed from the literal interpretations and that the sharper the

[40] This idea becomes clearer if one considers poetic metaphors which, although following the same pattern as simple expressions, can still reverse the mappings. Thus, Kipling's "dawn comes up like thunder" compares "dawn", which under standard interpretations is low in brightness and, therefore, in loudness, with "thunder", which is high in loudness (though not usually judged very bright).

literal distinctions are the stronger are judgements about their metaphoric counterparts (the perceptual values of "sunlight" and "moonlight" are established earlier than those of "cough" and "sneeze", and therefore the former two nouns are easier to judge both in literal and metaphorical compounds). The experiment also confirmed the existence of a previously observed asymmetry between the visual and auditory modalities, as auditory expressions suggest visual parallels more easily than visual expressions suggest auditory parallels (for example, "high-pitched sunlight" being easier to understand than "bright sneeze"). Finally, there was noticed adjective dominance in compound expressions, and adjectives were also easier to translate across modalities than nouns: "high-pitched" and "low pitched" being easier to judge with respect to brightness than "sunlight" and "moonlight" with respect to pitch (see pp. 27-28 for the summary).

The second experiment compared loudness with brightness, and, as in the previous case, it was clear that children of all ages have a good grasp of this perceptual similarity. Similar to the observations concerning the first experiment, it was also suggested after the second experiment that the ability to understand metaphorical cross-modal comparisons depended on whether children could make adequate literal distinctions. As in the previous case, the comparison of loudness and brightness indicated adjectival and verbal dominance in the interpretation of compound expressions: children's judgements of "dim thunder" and "bright whisper" tend to reflect the difference between the adjectives not the nouns (see p. 50 for the summary). Generally, the cross-modal similarity between loudness and brightness was traditionally understood in psychophysics by ascribing to both of them the universal sensory attribute of "intensity", and this pair was most extensively studied. Marks et al. suggest that the fact that both loudness and pitch should stand in the same relation to brightness can be explained through their corresponding neural coding:

> "Perhaps both loudness and pitch resemble brightness to the extent that the temporal properties of neural activity in the auditory nervous system 'match' those in the visual system."(1987: 73)

Finally, the third experiment sought to establish the similarity of pitch and size. Unlike the other two experiments, this one showed that the cross-modal equivalence between pitch and size is not systematically understood by younger children, but becomes consistent only by the age of 13. As in the first experiment, it was noticed that there is a significant asymmetry between the acoustical nouns and the size nouns, because the differences in pitch were easier to translate into differences in size than the other way round. Interestingly, although adjective dominance was detected again, it covered literal, but not metaphoric meanings (see p. 66 for the summary). The third experiment also had a second half whose

goal was to find out whether the similarity between pitch and size is revealed in colour names. The assignment of pitch and brightness proved to be consistent enough among children, as well as among adults, where "yellow" was judged the highest in pitch and brightness, while "blue" was associated with low-pitched and dim. The conclusion was that the metaphorical attribution of pitch to colours derives from intrinsic differences in brightness because the ordering of colours with respect to their pitch followed their ordering with respect to brightness.

Generally, the results of the three experiments summarized above seemed to confirm Marks et al.'s expectations that the perception of cross-modal similarities is universal, systematic, and evidently present in early childhood. The ability that children demonstrated in interpreting pitch/brightness and loudness/brightness cross-modal metaphors also pointed in the same direction. Clearly, the asymmetries noticed need an explanation, and, after outlining Cytowic's views on synesthesia and the neurological basis of synesthetic and cross-modal perception, I will address Marks et al.'s general conclusions about how perceptual experiences are correlated with what they call verbal knowledge.

3. 3. Cytowic on synesthesia.

Cytowic (1989a) argued convincingly against equating synesthesia as a particular genetically mediated aspect of perception with cross-modal matchings made by non-synesthetic subjects. Although some studies show that synesthetic percepts usually follow the patterns of cross-modal associations such as those studied by Marks et al. - like louder sounds being matched with brighter and larger photisms during synesthesiae - the irreducible difference between the two is that synesthetic individuals "see" shapes and colours when hearing sounds[41], while non-synesthetic individuals only think that loud sounds correspond better to bright colours than to dim colours. Cytowic undertook a long study of synesthetic perception (by synesthetic individuals), and I will briefly outline those of his observations that disagree with Marks et al.'s assumptions.

Thus,

---

[41] As in a synesthete's report quoted by Cytowic: "The only real problem is that when I am driving and a very loud sound comes on... it is hard to see. The image intensity is directly proportional to the sound level. People laugh when I say 'turn that down, I can't see where I'm driving.'" (Cytowic 1989a: 51). Seeing is the way how Cytowic describes the experiences of synesthetic subjects. Perhaps, the word "hallucinate" may be more appropriate here, however, the difference between indeed seeing real world objects and having object-like visual experiences in the absence of objects causing them does not bear much on the present discussion.

> "[f]ound predominantly in women, synesthesia is a rare condition in which stimulation of 1 sense produces an involuntary perception in another sense." (Cytowic 1989b: 849),

where "involuntary" is one of the major criteria that distinguishes true synesthetic perception from simple associations[42]. Cytowic discusses 42 subjects, all of whom reported having the specified condition. He points out that in synesthetic perception any combination of senses is possible, or even multiple combinations of senses as in the case of Luria's (1968) famous patient, who responded to auditory stimuli with parallel visual, tactile and taste sensations. The most common case of synesthesia is coloured hearing or chromesthesia, and colour generally figures prominently in a variety of synesthetic experiences, ranging from coloured letters to word-, number-, name-colour associations, coloured olfaction, coloured taste and coloured touch (see Cytowic 1989a: 26). Other synesthesiae that Cytowic's subjects manifested included musical tastes and smell, visual smell, perceptions of shaped pain, audioalgesia, and polymodal synesthetic perceptions.

Cytowic's observations of the subjects did not show that there were any obvious correspondences either between stimuli that evoked the same responses across synesthetic individuals or between their parallel responses to a stimulus. Thus, coloured hearing as a synesthetic perception does not necessarily follow the universal pattern of correspondences between pitch and colour suggested by Marks et al in their third experiment[43]. Nonetheless, there are five common features of perceptual experiences shared by synesthetes and missing from cross-modal associations of non-synesthetes (see ibid.: 64-65).

First, synesthetic perception is involuntary, although it may be elicited by particular stimuli; the parallel synesthetic sensation is perceived as a natural part of these stimuli, not as a distinct sensation. Second, synesthesia is projected, i.e., it is perceived as external rather than imagined, unlike simple cross-modal associations of non-synesthetic individuals. Third, synesthetic percepts are durable and discrete, they do not change over the lifetime of an individual, their range is restricted and they are unelaborated. Fourth, synesthetic percepts are memorable and to many synesthetes they serve as mnemonic devices. Fifth, synesthetic

---

[42] That may imply that simple associations made by non-synesthetic individuals are "voluntary", the view that is not entirely justified in view of the evidence collected by Marks et al. I raise this question again at the end of the section

[43] Jakobson (1968) remarks on the existence of phenomenal similarity between sound and colour systems, noting the tendency of the vowels *o* and *u* to be associated with dark colours, and the vowels *e* and *i* with light colours. However, more extensive reports by Cytowic on synesthetes' systems of colour associations show that generalizations of this type may be problematic. Whether there are any important analogies between the development and pathologies of the colour system and the phonological system, also suggested by Jakobson (see p. 84), remains to be ascertained as Cytowic says nothing on the subject.

perceptions are accompanied by strong emotional feelings, and synesthetes generally say that they would not want to lose their synesthesiae.

According to Cytowic these five features also distinguish idiopathic synesthesia from acquired synesthesiae, such as drug-induced synesthesia, etc. It is interesting to note in this respect that Marks et al.'s idea of synesthesia as a manifestation of universal cross-modal similarities was partially based on the observations that non-synesthetic individuals under the influence of psychotropic drugs have experiences similar to those of synesthetes (see p. 4). I will not discuss here Cytowic's views on this issue in detail; suffice to say that only in one of Cytowic's subjects diagnosed with idiopathic synesthesia the intensification of his spontaneous synesthesia was observed under the influence of LSD. Other subjects who had taken LSD in the past reported having no synesthesia with the drug. In short, while the performances of non-synesthetic individuals under the influence of LSD is reminiscent of synesthetic perception, and while LSD produces the suppression of cortico-cortical connections similar to the suppression of cortical activity during synesthesia, there is not enough evidence to link conclusively the two phenomena:

> "Ideas on possible neurotransmitters involved in synesthesia are deductions based on historical and animal work, and are highly speculative. It is premature to take such speculations beyond a level that might suggest a theoretical structure to consider as a mechanism for synesthesia." (Cytowic 1989a: 93)

What may be more significant for our present discussion is that there are qualitative differences in the distribution of responses to a stimulus between synesthetes and non-synesthetes. Cytowic conducted two experiments with non-synesthetic and synesthetic subjects on the perception of geometric shapes in response to different simple and complex tastes, and on coloured hearing in response to single piano notes (a gustatory and an auditory synesthete compared each to three controls). The results of these two matching tasks showed that synesthetes chose only some restricted areas of the response domain, while non-synesthetes tended to include in their responses all available options. Moreover, one should not forget that for synesthetes their percepts are real, and what they perceive is usually very simple, like simple geometric forms, simple textures and tastes. Non-synesthetes, on the contrary, employ a variety of strategies in matching tasks, which may vary from acting according to one's intuition to following some logical scheme. It is interesting to recall in this respect that one of Osgood et al.'s subjects, in response to tones getting louder and then softer, drew a complex picture of a car approaching and then moving away. For Cytowic, it is evident that what Osgood et al. tested was not synesthetic perception.

Another result of these experiments distinguishing synesthetes from non-synesthetes is that synesthetes' responses show a clear asymmetry with respect to different groups of particular stimuli from the stimulus domain. That is, there are both absolute and relative effects in synesthetic perception (both a restricted repertoire of responses to one half of the stimulus domain and a full range of responses to the other half of the stimulus domain were observed). And thus, from the observations described above, Cytowic concludes that synesthesia is a phenomenon distinct both from ordinary mediated associations, probably achieved through shared abstract meanings, and from direct linkage between sensory channels, which would require absolute effects for every correspondence between stimuli:

> "Its distinctiveness appears to involve an intermediate level of stimulus-response association in which the stimulus-response mapping is neither completely one-on-one nor richly one-on-many. A combination of absolute and relational effects is shown, lending further support to the notion of an intermediate level of stimulus-response mediation." (1989a: 90)

It is clear that the results of Cytowic's experiments demonstrate that simple cross-modal associations are qualitatively different from synesthetic perception. It is also clear that none of the five criteria of synesthetic perception can be applied to subjects in Marks' experiments. However, there is one important point Cytowic says nothing about: what, if any, is the possible relation between the specific cases of synesthetic perception and generally made cross-modal matchings? Cytowic claims that the non-synesthetic controls in his experiments made their choices consciously and thus experientially denied having synesthesia, but there is some unclearness as to what counts as a conscious decision in a stimulus matching task. Interestingly, one of the previously selected controls refused to participate in the experiment on the grounds that there was no logical way for tastes and shapes to go together (see ibid.: 76), - this was evidently a conscious decision. But if a subject decides to associate sweet tastes with round shapes and sour tastes with conical shapes (the latter association was also characteristic of the gustatory synesthete), and does not give any clearly articulated reason to explain his choices, then there is something more to cross-modal associations than a conscious decision. And if there is a general tendency for associating tastes and shapes in this and no other way, then, perhaps, the similarity of choices has to do not only with shared abstract meanings but some common perceptual apparatus as well. Anyhow, what could otherwise explain a subject's deliberate association of sweetness with roundness? Generally, although there are strong grounds for accepting Cytowic's clear-cut distinction between synesthetes and non-synesthetes, the relationship between the two types of perceptual associations may need further investigation.

3. 4. Synesthesia, cross-modal equivalencies and the brain.

As is probably already clear from the previous section, Cytowic holds that synesthesia has very little to do with language or abstraction. And as his observations of synesthetic subjects suggest, synesthesia is a very concrete polymodal association, the explanation for which has to be sought not in the neocortex, but at the earlier stages of neural processing. I will presently restate Cytowic's suggestions as to where in the brain one should locate synesthesia, but one is asked to keep in mind that these suggestions are not always very clear or conclusive (cf. Humphreys 1990).

Thus, Cytowic argues that synesthesia

> "... (1) is in the left hemisphere, (2) is not "cortical" in the conventional sense, and (3) involves temporal lobe-limbic structures." (1989a: 91)

That is, the neuroanatomical locus of synesthesia is below the level of cortex, in the limbic system; during synesthesia the cortical activity is suppressed, while limbic processing is enhanced. He suggests that derivative aspects of sensation are combined in the paralimbic or limbic areas, and that the limbic system provides the sense of conviction and the strong emotional character of synesthetic perception. The evidence for reduced cortical activity during synesthesia was obtained through the measurement of regional cerebral blood flow in the gustatory synesthete, and these findings were confirmed by Schiltz et al. (1999), who stressed the role of prefrontal regions and cortical inhibition in multisensory integration.

What Cytowic finally offers is the hippocampus model for synesthesia, which is based on the assumption that there are not many possible locations in the brain for bringing together signals from functionally different and independent areas. Thus, the anterior temporal lobe, which "virtually assembles every sensory modality", is closely functionally connected to the limbic system, and the information fed into the medial aspect of the temporal lobe is accessible to the hippocampus via the entorhinal cortex. The hippocampal formation can then respond back to every element that is fed into it, thus acting as a device responsible for monitoring "the fundamental goals of the organism as a biological entity" (see 1989a: 175).

In other passages, however, Cytowic says that that the existence of supposedly modality-specific cells that respond to other modalities may be the essence of synesthesia. He remarks that although there is a tendency for the separation of functions as one ascends the evolutionary scale,

> "the most important connections of association cortex are with the neocortex of the temporal lobe, which in turn feeds into limbic structures. Connections with any *one* sensory modality and the limbic system tend to be powerful in their arousal of emotional and autonomic responses, whereas nonlimbic sensory-sensory connections are weaker in this regard." (1989a: 159)

Cytowic admits that before synesthesia is localized with certainty and a definition of the limbic system is agreed upon, it is difficult to say anything about the relation of synesthesia to sensation generally. Nevertheless, he suggests that synesthesia may be considered a fundamental quality of sensation, which became less and less significant with the evolution of the mammalian cortex and the separation of the associative activities from the receptive activities. In this sense, synesthetic perception may be a result of a common mammalian fusion of senses which may still exist but which "never arises to consciousness" (see also Kesner (2000) on mammalian continuity in terms of prefrontal cortex function).

In the next section, I will mention Cytowic's idea of synesthesia as a disconnection of language areas from the limbic system. For now, it is evident that Cytowic's position is in need of further clarification, since, on the one hand, synesthetic perception cannot be considered of the same type as non-synesthetic associations between senses, while on the other hand, Cytowic is sometimes prone to consider synesthesia as a fundamental perceptual process. It is probable that the major difference between Cytowic's and Marks' consideration of synesthesia is that Cytowic takes it to be the fundamental quality of sensation as separate from language, while Marks takes it to be a part of the universal associations between senses apparent in the use of language as much as in the synesthetic perception itself. I will return to this issue later again, after having discussed Marks et al.'s views on universal properties of cross-modal associations, their possible origins in the brain and the nature of perception.

As stated earlier, Marks et al. (1987) intended their experiments with children as a confirmation of the idea of a common core of cross-modal similarities. In accordance with their expectations, the experiments showed that even very young children were consistent in perceiving and judging similarities between stimuli pertaining to the visual and auditory modalities. Moreover, there is also evidence that 3-week-old infants respond to the "similarity" between loudness and brightness (Lewkowicz and Turkewitz 1980), that one-month-old infants form cross-modal associations between visual and "oral" modalities (Meltzoff and Borden 1979), and that 1-year-old infants are sensitive to several auditory-visual correspondences (Wagner, Winner, Cicchetti and Gardner 1981)[44]. Since these

[44] Kagan (1985) remarks in passim that infants can detect the similarity between a discontinuous sound and an interrupted line, which form two different perceptual inputs, and uses this evidence to argue against Fodor's (1983) modularity hypothesis on the grounds that modular systems are holistic.

findings suggested that there are enough reasons to believe synesthetic perception (or the ability to perceive cross-modal equivalencies, which they call synesthesia) universal rather than idiosyncratic, there are reasons to consider it as an innate property of perception.

An alternative hypothesis would be that cross-modal similarities are derived from experience, by associating the observed regularities in everyday life. However, they note, it seems very unlikely that co-occurrences of brightness and loudness, for instance, should produce a universally reliable connection. Moreover, this hypothesis can explain the difference in developmental timetables, i.e., the fact that the brightness-loudness link appears much earlier than the pitch-size link, only by appealing to the salience of the former relation, which does not seem likely either. And generally:

> "Even if experience does teach us to associate loudness with brightness or pitch with size, it is much harder to specify a source for an 'ecological' connection between pitch and brightness. The association between relatively high pitched sounds and bright lights or light colors seems not to have an obvious external source. But it surely has an early developmental origin." (p. 72)

Correspondingly, Marks et al.'s own explanation of why the regular connections between pitch and brightness, and between loudness and brightness are stable by the age of 4, while the pitch-size similarity becomes established only by the age of 11 or so, is that the former are innate intrinsic characteristics of perception, but that the latter, although also an intrinsic property, is acquired from experience by associating large objects with low frequency tones and small objects with high frequency tones.

Thus, it is evident that cross-modal similarities studied by Marks et al. are psychologically real, even though there may be no simple or one-to-one relation between attributes of visual and auditory modalities. To explain how the similarity relations among different modalities are possible Marks et al. discuss two models of the structure of perceptual dimensions. In the first model, direct correspondences between pitch and brightness, between loudness and brightness, and between pitch and size are provided by reducing the dimensions of one modality to the attributes characterizing another modality. Thus, since psychophysical research identifies "volume" and "density" as additional attributes of the auditory modality, and since volume can be identified with auditory size,

---

I will not take up further the issue of cognitive architectures in much detail, but would like to draw the reader's attention to the fact that the existence of cross-modal transfers does not necessarily imply the existence of perceptual-cognitive continuity. Thus, as an example of cross-modal linkages, in the *Modularity of Mind* Fodor discusses the McGurk effect (the induction of hallucinatory speech sounds in subjects when they are presented with "a visual display of a speaker making vocal gestures appropriate to the production of those sounds" - p. 132), showing that the effect itself is domain-specific (language specific), as the visualization of a bouncing ball does not induce any sound hallucinations.

the pitch-size similarity becomes a correspondence between auditory and visual size. Similarly, since density characterizes both loudness and pitch, pitch-brightness and loudness-brightness equivalencies are reduced to density-brightness similarity, and density itself may be understood as "auditory brightness".

However, Marks et al. are not ready to accept this simple model because of the evidence obtained in some previous experiments, which did not support the existence of a unitary dimension of density or brightness, which would be common both to hearing and to vision. The second model, which in Marks et al.'s opinion is probably more accurate, represents the primary attributes of visual and auditory modalities as distinct from one another, and assumes that cross-modal associations involve many-to-one mappings from one modality onto another. Thus, as mentioned in section 3.2, this obtains in the case of the multiple mapping of both pitch and loudness from the auditory modality into the single attribute of brightness in the visual modality.

So, Marks et al.'s general idea is that because resemblances across perceptual domains cannot be properties of objects given in experience, they must be a property of perception itself, and this can only mean that intrinsic, nonderivative perceptual similarities most probably reside in some common neurophysiological codes for perceptual attributes (such as intensity) of the auditory and visual systems[45]. Similarly, Marks and Bornstein (1987) comment on the results of the experiments discussed above that one way to interpret the evidence that even young children are aware of cross-modal equivalencies is to suppose that some of these connections are "built into" sensory systems either by common neural coding or by the sensitivity of single cells to information received from different modalities. They remark later (after Wilson 1965) that there can also be other mechanisms mediating the perception of similarities across domains:

> "First, it is known that some cortical neurons are polysensory; that is, they respond in similar ways to specific inputs from more than a single sense modality. Second, it is known that information from modality-specific cortical projection regions is integrated at particular cerebral association areas; cortical damage in these areas results in deficits in cross-modal perception. In short, there are several types of mechanism identifiable in the central nervous system that could subserve cross-modal similarity, and, insofar as they may be wired innately to do so, would enable the young infant to match across modalities, in the absence of learning or experience." (ibid.: 58).

To summarize the results of the previous sections. It seems that we can take for granted the following three statements: a) synesthetic perception is not the same as an ability to

perceive cross-modal equivalencies; b) the ability to perceive cross-modal similarities[46] is universal and, most likely, innate; c) synesthetic and general cross-modal perception may have a common neurophysiological basis, although nothing definite can be said about this presently. And thus, with this in mind, we can now move on to the discussion of whether perceptual similarities are manifested in a systematic way in semantic structures.

3. 5. Synesthesia, cross-modal associations and the language faculty.

This is the most interesting part of the whole story about synesthesia and cross-modal connections, but it is also the most difficult one. This time I will start with describing Marks et al.'s general position on how cross-modal similarities are manifested in language use. I will then show that their explanations are not always satisfactory by discussing a number of issues raised either in the course of their experiments or in their theoretical conclusions. Finally, I will return to Cytowic's objections to the idea of semantic mediation of synesthetic perception mentioned in the previous sections, and discuss an alternative view of the relation between language and cross-modal associations.

3. 5. 1. Marks et al. on language and perception.

One can say that Marks et al.'s position on the relation between perception and language is not all that clear, and that it amounts mostly to the postulation of an "implicit verbal knowledge of cross-modal similarities". The recurrent idea seems to be that verbal processes gain access to perceptual knowledge, and that language, therefore, incorporates perceptual knowledge or that "verbal (semantic) knowledge taps perceptual knowledge" (p. 77). One also gets the impression that what is meant by perceptual knowledge is the structure of perceptual domains (organized in a continuum according to the properties of received stimulations) as consciously or unconsciously accessible to the perceiving organism, whereas verbal knowledge is the conscious knowledge of what labels properly designate different dimensions of perception.

---

[45] Bekesy (1959), to whom Marks et al. refer at one point, showed that there were a number of similarities between hearing (loudness) and the skin sensations (also vision and the skin sensations), in particular the analogous patterns of neural inhibition observed in the two systems.

[46] Note that what I propose here to take for granted are purely perceptual similarities, not linguistic associations that may express them.

Verbal knowledge of cross-modal similarities is therefore the knowledge of what sorts of labels can be substituted one for another in order to comply with the rules of cross-modal perception, and for this reason it is implicit. One can do no better than to quote from Marks et al. in answer to the question of how the connections between verbal and perceptual knowledge become established. Thus, they believe

> "that perception and conception of cross-modal similarity are closely connected. We view the resemblances... as quintessentially perceptual... and we believe that they in turn become available, perhaps automatically, to the more abstract system of knowledge embodied in language. One end result of the extension of perception to language is the capacity... to interpret cross-modal expressions in a systematic manner; but this capacity, even when based on innate cross-modal correspondences does not emerge as soon as children establish verbal labels for the pertinent aspects of perceptual experience... (p. 74).

The representational part of the story is that the structure of perceptual dimensions built upon the principle of alternate polarities and gradients (see section 3.4) is paralleled in the multidimensional conceptual space. This exotic expression means only that the establishing of perceptual cross-modal correspondences (their order, gradations and direction) is accompanied by establishing gradations in sensory meanings. Roughly, if children judge "bright sound" louder than "dim sound", then they must have established the verbal correlate of the perceptual equivalence between 'loud' and 'bright'. The multidimensionality of meanings thus implies that in the course of development internal representations of sensory meanings are organized into graded continua (see p. 77).

It should be clear now what the role of cross-modal metaphors is supposed to be. Just as brightness is not a perceptual attribute of sounds, it is not a proper linguistic label in describing one's auditory experience (cf. Marks 1982a: 177). But, the reasoning goes, since cross-modal perception is psychologically real, since metaphoric translations across modalities are almost universally identical, and since these translations are not arbitrary ('loud' and 'bright' being equivalent in their intensity), it is natural to assume that perceptual knowledge is revealed in synesthetic metaphors (cf. Marks and Bornstein 1987: 58). Or else, that cross-modal similarities in language have the form of similes or metaphors (see Marks et al. 1987:1).

The developmental issue to be addressed here is that, since cross-modal metaphors are expressions of similarities intrinsic to perception, children's ability to understand and interpret them does not imply that they necessarily recognize metaphors as such. Marks et al. comment that there can be two answers to this, each supported by theoretical arguments: either children do not discriminate between perceptual categories the way adults do, and thus do not have criteria for metaphoricity, or they do know when they violate conceptual

boundaries, and thus can distinguish metaphors from literal uses. Marks et al.'s choose an intermediate position saying that cross-modal similarities become metaphoric "whenever the modalities become independent perceptual categories" (p. 82), and this is achieved by the age of 4.

Intuitively the whole story seems extremely appealing: it is so neat and simple that some metaphoric expressions should have a biological basis in the perception of phenomenological similarity. What is more, under Marks et al.'s interpretation, one does not need to get involved in unproductive speculation about perceptual attributes common to all sensory modalities. But I do not think that most of their suggestions about the relation between language and perception are acceptable. Some considerations to this effect appear below, and although they are organized into two groups, they lead pretty much to the same conclusion, but, as it were, from different angles.

3. 5. 2. Theoretical issues.

Marks et al. claim that verbal processes gain access to perceptual knowledge, and that the structure of the sensory conceptual space corresponds to the structure of perceptual dimensions:

> "... perceptual experiences and meanings are multidimensional, and development can involve both the creation of additional dimensions of meaning and the differentiation of holistic psychological entities into component domains" (pp. 74-75).

First, labels designating particular characteristics of experiences pertaining to different sensory modalities become established ("loud", "bright"). Then, together with the organization of elements of perceptual dimensions by their order, gradation and polarity, the conceptual space becomes structured in a similar way (together with the ability to evaluate auditory stimuli as to their loudness comes the ability to judge the relative loudness of words[47] like "squeak" and "thunder"). Finally, the capacity to understand cross-modal similarities depends on the establishment of a coherent system of linguistic labels appropriate for each sensory modality, whose elements can then be transferred across modalities according to their position in the conceptual space. But in any case, as Marks et al. stress, the verbal knowledge of cross-modal equivalencies itself is derived from implicit perceptual knowledge.

---

[47] Or, more precisely, their denotations.

Despite the general consistency of Marks et al.'s views, there are certain points in their position which I find problematic. They are summarized below.

1. *Sensory meanings are holistic.* First of all, what I find problematic with Marks et al.'s approach is that it requires "sensory meanings" (to borrow their expression) to be points on a mental rating scale. While it is true that knowing what "thunder" means somehow involves knowing that thunder is usually loud, it cannot be also true that it involves knowing that thunder is usually louder than a squeak. From the point of view of conceptual atomism, where the content of concepts is determined by their causal relation to the world, and where in order to have the concept 'thunder' one does not need to have the concept 'squeak', the inference from 'thunder' and 'squeak' to "thunders are louder than squeaks" is guaranteed by the absolute perceptual values of the referents of these concepts [48].

Under Marks et al.'s account, however, all sensory concepts belonging to the same modality are interdeterminable, which means that the relative perceptual values of "thunder" and "squeak" are part of the meaning of these words. And this may imply a vicious regress: unless one knows the loudness values of all auditory nouns, one cannot know what any of them means. Surely, this is not the case. Similarly for the modal adjectives themselves. While it is true that knowing what "loud" means somehow involves knowing that some auditory stimuli are louder than others, it cannot be part of the meaning of "loud" that drum notes are usually louder than piano notes, etc. (see p. 77)

Note that this is not trivial: thus, to give another example, "bright" does contribute its meaning to the expressions "bright sunlight" and "bright moonlight" just as well as "sunlight" and "moonlight" do, but it is not part of the meaning of "bright" that sunlight is usually brighter than moonlight. Speaking generally, conceiving of sensory meanings as multivalued or continuous (see ibid.) poses an irresolvable problem of compositionality: according to Marks et al. what is required for a good accomplishment of rating tasks is the establishment of prototypical sensory meanings of objects and events but, via Fodor (1998a), prototypes do not compose. That is, if the meaning of "sunlight" included a specification 'of the brightness X', then how could one evaluate "bright sunlight" against it? (Even if X is considered as a range, where "bright sunlight" means "sunlight at the high end of range X", one will come across a problem of determining where the high end begins.)

Consider also the example of "sneeze". Marks et al. stipulate that at certain stages, even though they are able to recognize the word's reference, children do not initially have a well-defined position for it in the "conceptual auditory space"(p.81). But does this show that they

do not have the meaning of "sneeze"? It is one thing to know what "sneeze" means/ refers to, but it is another thing to know that sneezes are normally high-pitched. So, it looks more like a question of establishing the meaning of "high-pitched" rather than "sneeze" that is reflected in the scale rating tasks.

2. *Perceptual characteristics are unambiguously mapped onto language.* Presumably, the tendency to represent meanings as organized around a scale has a lot to do with the observation that visual adjectives are easily judged with respect to their loudness and pitch. And the assumption is that since children and adults equally judge "bright" as louder and higher in pitch than "dim", then 'bright', 'loud' and 'high-pitched' stand in some non-arbitrary relation to 'dim', 'soft' and 'low-pitched', which is best thought of as a conceptual space paralleling the perceptual space. This observation is indeed important. But consider: if one has to judge "bright music" on the scale "loud - soft", clearly it will be closer to the "loud" end of the scale. However, "bright music" does not mean 'loud music', since it is possible to imagine that one describes music played softly as "bright" - the brightness of music seems to be a property of music, not a property of how it is perceived.

Or rather: naturally, it is a property of music as perceived (as long as we do not have a theory of what it is for music to be classified as bright), but one cannot change the brightness of the music played the same way as one can change its loudness. Thus, if the dependence of language on perception was as clear-cut as Marks et al. want it to be, this would imply that "loud" and "bright" are synonymous, the difference between the two being that the one is a literal label and the other is a metaphorical label for one and the same sense impression. Similarly, this would also imply that "high-pitched colours" and "loud colours" must be synonymous too, since, perceptually, brightness stands in the same relation to pitch as it stands to loudness. But as neither is the case, then clearly there is something more intricate going on here than mere verbal reflection of phenomenological similarity.

3. *Two-level explanation for cross-modal correspondences.* In section 3.3, when discussing Cytowic's experiments with synesthetic and non-synesthetic subjects, it was mentioned that synesthetic perception could not be a matter of simple cross-talk among sensory channels because the results of the experiments showed the presence not only of absolute, but also of relational effects. Similarly, Cytowic discarded the idea of commonality between synesthetic perception and cross-modal associations as the results showed qualitative differences in the range of responses of the two groups of subjects. By analogy, one can say that another problem in Marks et al.'s theoretical suggestions is that

[48] "Perceptual value" here means not only loudness, but other perceptual characteristics (pitch, sound pressure and spatial dimensions) which allow one to reliably identify thunder and squeaks under

they want a two-fold explanation of cross-modal equivalencies: a) through the innate multidimensional perceptual space and/or multimodal responsiveness of nerve cells, - i.e., they want cross-modal equivalencies to be a matter of universal properties of perception (although, one is asked to note that "the multidimensional perceptual space" is only postulated, no hint is given as to where the place for perceptual transfer is); b) through shared multimodal meanings, - i.e., they want cross-modal equivalencies to be a matter of the application of verbal labels or of verbal knowledge (see p. 6). Clearly, these two explanations cannot be complementary.

This problem was not resolved in a later work (Melara and Marks 1990). Melara and Marks (1990) came up with results that refuted the idea of cross-talk between different sensory channels, and suggested that the source of cross-modal equivalencies was in semantically based interactions (i.e., the way our language influences how we perceive correspondences between modalities). However, they admitted that semantically based interactions could not be present in early infancy. Thus, if the correlation between brightness and loudness is simply a matter of semantic mediation, the fact that infants can sense this correlation is left unexplained. The two-level explanation failed to be reduced to a consistent view.

4. *The origin of verbal knowledge.* With respect to the previous considerations, it is also interesting to note that Marks et al. (1987) - and for that matter, Melara and Marks (1990) - somewhat avoid the issue of the origin of verbal knowledge. They say that perceptual resemblances become available to the more abstract knowledge embodied in language. This can be understood as if they were saying that verbal knowledge gets abstracted from the relevant dimensions of experienced stimulations in different sensory modalities, but this would not agree with their idea of the multidimensionality and mutual influence between perception and language. In her discussion of Marks et al.'s monograph, Smith (1987) remarks that

> "[t]hese comments about the potential value of studying the structure of perceptual relations for understanding relational language should not be taken as support for the feature-by-feature model of acquisition. Words and their relations to each other may be organized by a feature structure, but children may map words only to fully evolved concepts, not to individual features. This possibility is pertinent to the finding in this *Monograph* of a consistent gap between the making of cross-modal correspondences in perception and language" (p. 99)[49]

---

favourable conditions.

[49] See also Carey's (1982) arguments against the feature model of semantic development.

Without agreeing or disagreeing with Smith, I think that there are some reasons to believe that Marks and colleagues may be closer to the nativist position about concepts than to an empiricist idea of explaining sensory concepts as emerging from sensory experiences. Thus, Marks and Bornstein (1987: 62) outline three theories explaining the origins of sensory comparison and integration, and conclude that the availability of cross-modal similarities early in life is most compatible with the nativist position, which assumes that senses are co-ordinated at birth. But if language parallels perception[50] in the manner Marks et al. want it to do, it should also be reasonable to conclude that sensory meanings are also "given" at birth, and that development consists in unfolding the innately given knowledge, perceptual experiences and their verbal equivalents being specified and graded simultaneously. Unless this is the conclusion one is supposed to arrive at, granted that Marks et al.'s assumptions concerning the parallel development of perceptual experiences and meanings, the theory they develop in the *Monograph* is not internally coherent.

5. *Final point: metaphors in perception.* Because of their tendency to equate concepts with implicit perceptual categories, Marks and colleagues are not always careful in their use of words. Thus, Marks (1982a) employs the apparently oxymoronic expression "synesthesia in language". Marks et al. (1987) say that cross-modal similarities become metaphoric when the modalities become independent perceptual categories, and thus, that "cross-modal similarities are not metaphoric to infants" (see p. 83). This is repeated in Marks and Bornstein (1987):

> "Clearly, some synesthetic metaphors are present in early childhood and seem not to require much experience in developing, whereas others emerge later and may be based on experience." (p. 55)

It might appear that here, almost opposite to what was said previously, metaphor is considered not as a way to express perceptual experience but as this experience itself, i.e., that the perceptual equivalence between pitch and brightness is itself a metaphor. This is how it was interpreted by R. Haskell in the introduction to the volume where Marks and Bornstein's article appeared:

> "The authors present findings from neurological data that they suggest as a model for the origins of sensory metaphor and as a model of the early developmental stage of linguistic metaphor" (1987: 3).

And thus, the study of cross-modal similarities begins to look like a good support for the cognitive linguistics theory of metaphor discussed earlier (cf. Sweetser 1990: 9 on

[50] At least as far as synesthetic expressions are concerned.

metaphorical structuring of "our perceptions of the world"). However, such a view would be inconsistent with what Marks et al. say in other places - that metaphor is a matter of the application of verbal labels (i.e., not a cognitive mechanism in itself). Because, clearly, nowhere else in their texts do they consider the idea that perception itself can be metaphorical (a digression: what would it amount to anyway, if one perceived certain things or events as if they were something else?). This is also confirmed by what Marks and Bornstein (1987) say a couple of pages before the quoted passage about young children's having an implicit perceptual knowledge of cross-modal similarities which parallels the implicit verbal knowledge of cross-modal similarities that "adults use in comprehending certain important kinds of metaphors" (see p. 53).

To sum up. In this section I attempted to show that: a) sensory meanings cannot be holistic; b) the cross-modal transfer of meanings does not necessarily reflect perceptual equivalencies; c) correspondingly, there is not enough reason to think that conceptual structures directly reflect cross-modal perceptual similarities; d) there are reasons to believe that conceptual structures corresponding to sensory modalities are innately specified; e) one has to be careful not to blur the boundary between perceptual cross-modal similarities and their linguistic equivalents.

3. 5. 3. Interpreting the results.

As mentioned above, if language followed perceptual equivalencies in a one-to-one manner (if "bright sounds" meant 'loud sounds' and "loud colours" meant 'bright colours', etc.), then the asymmetries observed in the experimental results should not have arisen. Let me give here two examples.

First, take the asymmetry between literal and metaphorical graphic representations of verbal stimuli. Marks et al. note that literal distinctions tend to be larger than metaphorical distinctions, since in children's judgements the difference between "moonlight" and "sunlight" is larger with respect to brightness than with respect to pitch. Similar results were obtained by Marks (1982a) in his experiments with adults - that the metaphorical effects are generally smaller than the literal ones:

> "The conclusion from these data is that visual nouns do not simply impart intensity; they do not automatically imply loudness by means of their brightness... Moreover, the modified words or phrases themselves did not, in any simple or systematic way, translate their loudness into brightness or their brightness into loudness. *Thunder*, though enormously louder than a *whisper*, was not at all brighter" (p. 184)

In 1982 Marks believed that brightness was a psychophysical attribute of auditory experience, and this would have explained the observed effect: thunders and whispers differ in their loudness more than they differ in their brightness, because brightness is a composite of loudness and pitch. However, Marks et al. (1987) do not think that this is true any longer. It follows that either a) brightness is some attribute of auditory modality that has to be formulated more precisely; or b) brightness is an attribute of visual modality, and the transfer of brightness to the description of auditory words and experiences rests in the semantic level, but this would not allow one to tell any plausible story about why there are still reliable correlations between brightness and loudness (see section 3.2). I think that there may be a third alternative (though, closely related to the first one): that the concept 'bright' is, as it were, amodal. But as this may appear strange, I will leave a detailed discussion of this issue till later.

Another asymmetry noticed by Marks et al. (1987) is that acoustic events are easier to judge with respect to their brightness than are visual events with respect to their loudness and pitch. The observation is also supported by the intuition that "bright sneeze" and "large thunder" are apt metaphorical expressions when "high-pitched sunlight" is a strained one. Marks et al. note that this asymmetry shows similarity with synesthetic perception where it is more common for auditory stimuli to invoke photisms than it is for visual stimuli to invoke sound impressions. Thus, they conclude that the source of this asymmetry may be in perception rather than in cognition:

> "The direction of asymmetry in synesthesia parallels that in verbal metaphor. If synesthesia is perceptual in origin and nature, as we suspect, then asymmetry in synesthesia would suggest... that asymmetry in metaphor too may reside in some as-yet-undetermined, presumably neurophysiological, characteristic of perceptual processing." (p. 81)

I have a couple of remarks to make concerning Marks et al.'s explanation of visual-auditory asymmetry. First of all, one has to remember that what Marks et al. mean by "synesthesia" cannot be taken for granted, and the relation between synesthetic perception and the ability to make cross-modal associations is not one they ever clarified. Second, one has to remember that the idea that verbal cross-modal equivalencies are derived from perceptual equivalencies was not confirmed, although this is not to deny that neurophysiological explanations may be relevant to explaining the ratings of verbal stimuli.

Consider now the following example: "soft sunlight" appears less metaphorical than "loud sunlight", which in turn appears less metaphorical than "high-pitched sunlight", but all three of them are visual events described in acoustic terms, as Marks et al. would presumably put it. However, it is not obvious that 'soft' is primarily an auditory concept

(one could have used instead "quiet sunlight"). Similarly for visual-auditory transfers: "large thunder" vs. "small thunder" are clearly more informative expressions than "bright sneeze" vs. "dark cough", although under the criterion of reflected perceptual correspondence they should be considered equally acceptable on all counts. Thus, to reiterate: the rating experiments may give an insight into the conceptualization of perceptual correlations, but they cannot be taken as testing the meanings of expressions[51].

Finally, I would like to comment briefly on the use of verbal stimuli in Marks et al.'s experiments. Describing the first experiment (see section 3.2), I mentioned that they observed asymmetry between the visual and auditory modalities in that auditory expressions suggest better parallels for visual expressions than the other way round. The examples compared in this experiment were "high-pitched sunlight" and "bright sneeze", where "high-pitched sunlight" turned out to be an expression easier to interpret. In this respect, it would be interesting to know whether using instead "bright voice" as a stimulus would have produced a different result for this type of comparison, especially as "sneeze" is not ordinarily a subject of any specified descriptions compared with voice (I do not mean that it is impossible to describe a sneeze – it may be loud, for example – but that we normally do not do it in our everyday life). I suspect that somewhat changing the stimuli in this and other cases would have eliminated certain asymmetries and unexpected results (cf. Smith 1987 on the use of perceptual stimuli in the pitch-size experiment; similarly, one could also question the use of verbal stimuli in this experiment). And thus, as we have already seen in the previous discussions, one has to distinguish the question concerning the meaning of "bright" from the question of cross-modal parallels induced by the comparable intensity of the stimuli.

3. 5. 4. Cytowic on synesthesia as dissociation.

As stated above, Cytowic (1989a) insists on a strict distinction between synesthetic perception and cross-modal associations, which are a matter of shared meanings rather then perceived equivalencies. He criticizes the previous approaches to the study of synesthesia,

[51] I will not go into this in much detail. Note, however, that this consideration is also valid for explaining why, as Marks et al. found, it is easier to translate adjectives across modalities than it is to translate nouns. It might be that translating adjectives - "high-pitched" being judged brighter than "low-pitched"- has to do with reflected perceptual correspondences as to the perception of intensity in different sense modalities and is therefore simpler than translating nouns - "sunlight" not being so readily judged to be higher in pitch than "moonlight" – where we find stronger connection with their

where it was assumed that the connections between a stimulus and the parallel sensation were brought about by linguistically-based associations, and shows that synesthetes rarely attach any meaning to their synesthesiae. In discussing Osgood at al.'s semantic differential, Cytowic remarks that what the authors in question tested in their experiments, in which subjects were required to draw pictures in response to rising and falling tones, were abstract concepts underlying the presented stimuli, not sounds. Thus, what Osgood et al. showed was that "stimuli from several modalities... *may* have shared significats or meanings" (p. 179), not that synesthesia has anything in common with semantic judgements. What's more, the experiments by Wood and Cytowic (see Cytowic 1989a: 182) suggested that the semantic differential does not consistently explain the matchings not only of synesthetic, but also of non-synesthetic subjects.

Cytowic emphasizes that the nature of language and cross-modal associations with respect to synesthesia have been misunderstood in that both synesthesia and cross-modal associations were considered as derivative aspects of language. Following Geschwind, he holds that, on the contrary, cross-modal associations are requisite for language:

> "The ability to acquire speech has as a prerequisite the ability to form cross-modal associations. In sub-human forms, the only readily established sensory-sensory associations are those between a non-limbic (i.e. visual, tactile or auditory) stimulus and a limbic stimulus. It is only in man that associations between two nonlimbic stimuli are readily formed and it is this ability which underlies the learning of names of objects." (Geschwind 1964: 155; quoted in Cytowic 1989a: 72)

Geschwind (1965) put forward a theory of disconnection to explain aphasias, agnosias and apraxias in terms of visual regions being disconnected either from the speech area, the somesthetic areas or the motor region (p. 596), and emphasized the importance of lesions of association cortex and association fibres in "producing disturbances of the higher functions of the nervous system" (p. 634). This is still an unrefuted hypothesis, whose explanatory value was either rejected by later researchers (e.g., Adair et al. 1997, Clark et al. 1994; see also Caplan 1987 for an overview), or supported by further observation showing its applicability to the explanation of certain phenomena (e.g., Damasio and Damasio 1980, Kertesz et al. 1982, Sierra and Berrios 1998, Starr et al. 1991, Masson et al. 1991). As such, there is presently not enough evidence to give Geschwind's theory a fully objective evaluation.

And thus, returning now to the issue that interests us here, in the first part of his 1965 article, Geschwind also emphasizes the point that the ability to develop speech in humans

---

meanings qua denotations (the property of pitch has little relevance for establishing the meaning of

depends on the establishment of non-limbic cross-modal associations (cf. the introduction of the angular gyrus region and the emergence of the parietal association area - p. 274), which are absent in sub-human primates. Further, Geschwind comments that difficulties of tactile-visual and auditory-visual transfers in monkeys (as demonstrated, for example, by Ettlinger 1960) do not show that monkeys form no cross-modal associations, but rather that all their associations are made to "limbic" modalities or reinforcers, while humans are capable of non-limbic associations[52]. Cytowic follows Geschwind in that he holds that it is on the basis of this ability to form intermodal associations, which is presumably found in synesthesia, that humans could develop speech and then form other intermodal associations, which constitute the most abstract cognitive functions, and with which synesthesia was often confused. He, therefore, suggests that cross-modal metaphor is a linguistic derivative of "the stuff of synesthesia" (p. 185), and proposes to view synesthesia as a disconnection from the language modules of the brain.

Unfortunately, this is all that Cytowic says on the issue. However, there is an interesting suggestion that may follow (at least, there is nothing to indicate that it does not follow) from Geschwind's idea of language as depending on stable cross-modal associations - it concerns the conceptual structure. And, roughly, it amounts to saying that it is the same psychologically primitive concepts that are employed in our understanding and representing properties belonging to various sensory domains, and this is why cross-modal metaphors are conceivable and so easily interpreted (cf. Marks et al. 1987: 84). What *this* leads to is the suggestion that cross-modal metaphors may not be metaphors after all. Or, at least, that not all of them are metaphors.

3. 6. Implications for theories of concepts.

This chapter was a continuation of *Evidence from Pain Research*, and thus it dealt with another group of expressions that are and are not metaphors at the same time. On the standard criteria of metaphoricity, "bright music" and "loud colours" are both metaphorical expressions because they violate conceptual boundaries. And they violate conceptual boundaries because brightness is a category proper to vision, while loudness is a category

---

"sunlight" or "moonlight").

[52] It is interesting to note that commenting on monkeys' inability to transfer learning across modalities, Sutherland (1973) remarks that "[o]ur ability to perform cross modality matching suggests that we store a description in terms of highly abstract structures and that we possess mapping rules that enable us to form the same description at this level from both a visual and a tactile input" (p. 173).

proper to sound (cf. Marks et al. 1987: 83). However, there is something peculiar about "bright music" and "loud colours" in that there is no precise, explicitly specifiable, similarity relation which would justify the metaphorical transfer, if this is what underlies the production and understanding of such expressions (I have attempted to show this arguing against Marks et al.'s idea of the parallel perceptual and conceptual spaces).

I suggest (intuitively for the moment, and I will try to defend this position in more detail in the following chapters) that nothing prevents one from saying that "bright" does not only properly describe stimuli in the visual modality, but also stimuli in the other modalities, even though the properties depicted by "bright" in "bright light" and "bright music" are not the same[53]. Better, the concept 'bright' that one entertains when thinking about bright music and bright lights is one and the same concept: this will not make "bright light" and "bright music" have the same meaning, because in one case "bright" is predicated of light and in the other case it is predicated of music, and music and light are not the same (and compounds need not mean the same either). However, to say such a thing is to irritate philosophers somewhat, as this suggestion runs contrary to certain ideas of truth and reference.

I am not so sure that it has nothing to do with either reference or truth, if reference is taken in a loose sense of what speakers of a language refer to (tend to refer to) when they utter "blah-blah-blah", and if there is a way to show that someone was using an expression erroneously. Thus, one can test the reference of "bright music" experimentally: if the overwhelming majority of speakers reliably apply "bright music" to some kinds of melodies and not others, this would show that "bright music" has a clearly defined reference, and is not just a matter of "whatever someone means by it" (and this is what I like about Marks et al.'s results - the fact that there are reliably the same types of responses to certain verbal

---

[53] One may object here that (intuitively again) speakers are inclined to think of some expressions as more metaphorical than other expressions; this would be the case of "bright light" as opposed to "bright sound" and "bright person". A lot here hinges on one's understanding of literalness and meatphoricity. Thus, in the cognitive linguistics tradition (see chapter 1), most language is metaphorical, but it is not judged as such by the majority of speakers (for instance, they do not recognize "the solution of my problems" as a metaphor – see Lakoff and Johnson (1980: 143) for a striking illustration of this).

To show that speakers' inclinations are indeed such on the standard criteria of literalness and metaphoricity (the ones assumed by Marks et al., where the notion of "proper predication" serves as a borderline), one will need to provide evidence that, both in two word collocations and in larger contexts, speakers consistently identify cross-modal expressions as metaphorical. However, there is counter-evidence to this claim, showing that speakers do not reliably judge metaphors in test-sentences as metaphors (e.g., Margalit & Goldblum (1994: 232) who say that from the point of view of classification of sentences the literal-metaphorical distinction is a theoretical, not "practical" one; see also Sperber and Wilson (1986) for the view that with respect to relevance there is nothing peculiar about metaphor when compared with loose uses of language).

stimuli). I presume that one could expect the subjects to agree more on "bright music" than on "pink music", and this shows that the two expressions cannot be treated in the same way.

Similarly, if someone names an auditory stimulus "bright", when the overwhelming majority of speakers labels it "dim", we may rightly consider this person to be mistaken about the use of the attribute "bright" for the auditory modality, whatever it is that makes the majority of speakers to associate the attribute "bright" with the given stimulus. (Note that commenting on "when" metaphor is, Marks et al. (1987) say that children understand metaphors as metaphors only after they have established what labels properly belong to what modalities. Before that stage, "bright sound" is not a metaphor for them, since the correlation is available perceptually. Thus, when we consider cross-modal expressions, the question of whether the label is applied literally or metaphorically is first of all the question of linguistic conventions).

Note also that when philosophers speak of literal and metaphorical meanings of an expression, they often appeal to dictionaries to support their views. It is interesting to remark in this respect that what I have suggested above is precisely how dictionary entries are organized if one leaves out numbers and *fig.* labels: entry /of ... / definition/ examples or usage information. Consider the following extract from the OED entry on "bright":

bright
1. Shining; emitting, reflecting, or pervaded by much light
a. said of luminaries (1000)
b. of polished metals, precious stones, and other objects whose surfaces reflect light (1000)
c. of illuminated surfaces, of the day in sunshine, etc. (1000)
d. of transparent substances: Clear, translucent (1709)
[note that d. sense is not labelled figurative]...
2. Clear or luminous to the mental perception (1000)...
4.a. Of vivid or brilliant colour... (1375)...
5. Of sounds: **a.** Clear, shrill, ringing. **b.** *Said of the mental effect of a note (1000)* - my italics.

Clearly, dictionaries cannot be the final judges of meanings[54].

Finally, some remarks about the possible status of metaphor in perception. Unless one subscribes to the cognitive linguistics view (see Lakoff and Johnson 1999 on primary metaphors), it would seem that the idea of metaphor in perception is so obviously false that one should never have thought of it: we do not perceive something as if it were something else, similarly, why should we think of something as if it were something else, as this would have no apparent survival value? However, other theorists found it appropriate to speak of perceptual metaphors. Thus, discussing Marks et al. (1987), Smith writes:

[54] There are cases even more interesting and puzzling than "bright" - they are predicates that do not clearly belong to one sensory modality, for instance, "dry" or "soft". While one might argue

> "The central contribution of this work is the specification of a perceptual basis for certain metaphors. The developmental story about metaphors offered in the *Monograph* rings true to me. The authors suggest that metaphors such as 'loud lights' might not actually be metaphors early in development. Instead, 'loud lights' may simply be a statement concerning a directly perceived similarity. Perceptual 'metaphors' may become metaphors only with development and with the organization of specific sensory dimensions as dimensions. Not until loudness and brightness are organized distinctly can one be used to illuminate the other metaphorically." (1987: 98)

It is not accidental that Smith has to put one of her "metaphors" in quotes. At the time when she was writing this, there was enough evidence to the effect that sensory systems are specified at birth (e.g. Freides 1974, Fodor 1983), so this renders the talk of perceptual metaphors, even if in quotes, completely meaningless. The later talk of their becoming metaphors is similarly deprived of sense, for it is not clear how and for whom "loud lights" is first a statement about directly perceived similarities, and then a metaphor.

In this chapter I have attempted to show that the so-called synesthetic metaphors, or verbal expressions of cross-modal similarities, may not be metaphors after all. Assuming that cross-modal metaphors exist in perception as such (see previous paragraph) leads to a thorough muddle of trying to explain at what stages and for what reasons they should be considered either direct perceptual effects or effects of metaphorical transduction (to use Fodor's 1983 term). On the other hand, assuming that cross-modal metaphors appear in development only after the proper linguistic labels for the modalities have been established, and given that before that stage they are not metaphors but amodal statements of perceptual equivalencies, depends far too much on the notion of "proper labels". Whether these are justifiable grounds for discriminating between the literal and the non-literal will become the topic of the last two chapters. However, presently I will move on to discussing the last piece of evidence – associations made not between sensory modalities, but between physical and psychological predicates, as manifested in cross-linguistic, developmental, psycholinguistic studies and research on brain-damaged populations.

---

persuasively that "bright" is about vision, it is not clear that this could be done for these two examples.

Chapter 4. Evidence from double-function terms (I).

4. 1. The Puzzle.

In this chapter I would like to dwell upon the semantics of double-function terms (in particular, adjectives, such as "sharp" in "sharp knife" and "sharp person", with the possibility of extending the latter function to more abstract entities than characteristics of personality, such as events, intentions or products of human activity). Solomon Asch, who was one of the initiators of the whole inquiry back in the 1950s, once remarked that

> "There seems to be an outstanding fittingness in speaking of 'bitter truth' or 'dark purpose'. So much is this the case that these models of expression have for us every mark of being literal. [continued from the footnote to the last sentence] For this reason it may appear unjustified to consider this linguistic phenomenon as an instance of metaphor. Scholars have indeed reserved the term for cases of explicit comparison. From the psychological standpoint, however, this distinction does not appear compelling. The physical-psychologistical terms may in fact contain the clue to the process fundamental to metaphors." (1955: 30)

Similarly, in "Metaphor" John Searle speaks of certain types of metaphor, whose interpretation does not depend on the usual perception of some similarity between the referent of the tenor and the extension of the vehicle, but which seem to be simple facts about our sensibility.

> "Temperature metaphors for emotional and personal traits are in fact quite common and they are not derived from any literal underlying similarities... Similarly, taste metaphors for personal traits are not based on properties in common. We speak of a "sweet disposition" or a "bitter person", without implying that the sweet disposition and the bitter person have literal traits in common with sweet and bitter tastes which exhaust the utterance meaning of the metaphorical utterance. Of course, sweet dispositions and sweet things are both pleasant, but much more is conveyed by the metaphor then mere pleasantness." (1979: 98-99)

Later, in his book *Intentionality*, the existence of non-representational mental capacities is postulated in order to explain the factors underlying the interpretation of such metaphors:

> "It just seems to be a fact about our mental capacities that we are able to interpret certain sorts of metaphor without the application of any underlying 'rules' or 'principles' other than the sheer ability to make certain associations. I don't know any better way to describe these abilities than to say that they are non-representational mental capacities." (1983: 149)

With the development of cognitive linguistics, other types of metaphor (we shall call them that for the present) won attention. Eve Sweetser is among the few theorists in the cognitive linguistics tradition who directed part of her research to the global correspondence between the descriptions of internal and external self, as manifested in perception verbs, from the cognitive linguistics framework. Thus, she writes:

> "I will begin by applying a cognitive-semantic approach to a well recognized set of Indo-European etymologies in the hope of explaining a hitherto mysterious fact: certain semantic changes occur over and over again throughout the course of Indo-European and independently in different branches across an area of thousands of miles and a time depth of thousands of years." (1990: 9) and "The mappings I will be examining are examples of the sort of metaphorically structured, non-objective connections between senses..." (ibid.: 27)

In the next section of this chapter, I will describe Asch's results and conclusions, and attempt to show the significance of his considerations. For the moment, suffice to say that the observed physical-psychological correspondences present an interesting case for semantic study, even though it is usually excluded from traditional semantic analyses. Not necessarily being a clue to all metaphors, the clarification of the issue may tell us something about what is to be considered metaphorical and what is not. In this sense, it may be worthwhile to revive the long-forgotten discussions of almost half a century ago, especially as digging into the past has so often proved fruitful.

I will then, in this chapter, discuss some contemporary research on those polysemous adjectives that have both a physical and a psychological meaning, although this topic has never received much attention. The reports I am going to discuss include psycholinguistic study of the interrelatedness of meanings, and the processing of alternative meanings of words by normal and brain-damaged subjects. I will conclude the two chapters by considering whether the results of this discussion agree with what I suggested previously with respect to synesthetic expressions.

Formulating generally my position towards the study of cross-modal associations and double-function terms, and its possible effects on the study of mind, my pursuit will be guided by a well-recognized idea of language as a biological system. In this respect, I mention here Li's (1997) analogy between language and other biological systems, such as eukaryotic cell division and renal filtration. It states that, as in the study of these systems, in studying language it is better to avoid redundant explanations in favour of subsuming different phenomena under more general principles. That is, because language (the physical internal structure of language faculty) as we study it now is a "black box", it may also be

useful to recognize "a general function F of a system in the face of F's different manifestations" (p. 176)[55].

So, this is the black box I have in front of me now: as the input we have information from sensory modalities, plus some innately specified constraints on perception, knowledge, and the language faculty, plus, perhaps, memories from previous experiences; as the output we have a somewhat metaphorical way of speaking, which covers psychological reality as well as it covers physical reality (similarly for other conceptual metaphors). The question therefore is: what should be going on in the minds of individual speakers, so that they find it natural to use the same predicates for rocks, sticks and their neighbours regardless.

Let us make another use of the black box analogy: it appears that, there being nothing to show the contrary, mappings across sensory domains and mappings across some qualitatively different domains with respect to how they get expressed in a language (for example, whether you predicate "bright" of sounds or humans' mental capacities) must be explained by the same general function F, which is *not* metaphorical conceptualization. That is, if it is shown that cross-modal associations are not exactly metaphorical (and I hope to have shown this in the last chapter), then there may be reasons to believe that physical-psychological associations are not entirely metaphorical either - that there are better grounds for this similarity of expression than mere lack of words. Thus, keeping all this in mind, I begin now by recounting Asch's research on double-function adjectives.

4. 2. Asch on Double-Function Adjectives.

Solomon Asch's inquiry into double-function adjectives was a somewhat unexpected result of his previous research into the formation of impressions of other persons via linguistic descriptions, where the overwhelming usage of terms relating both to physical and psychological properties became evident. This observation provoked a study (Asch 1955) in which a number of double-function adjectives were compared across historically remote languages. These terms included: "warm", "cold", "hot"; "right", "left"; "dull", "bright", "pale", "shining"; "straight", "twisted", "crooked"; "sweet", "bitter"; "colourful", "colourless", "white", "black"; "rough", "smooth", "slippery"; "dry", "wet"; "clear", "cloudy"; "deep", "shallow"; "high", "low"; "broad", "narrow"; "rounded", "sharp"; "hard",

[55] Although, Li draws this analogy to show that Chomsky and Lasnik's (1993) principles and parameters model is not a biologically viable theory - "the ungrammaticality of an expression should not be accounted for separately by two UG principles" (p. 170) - I thought it was appropriate to use it in a semantic context too.

"soft" (p. 31), and the languages compared to English were Old Testament Hebrew, Homeric Greek, Chinese, Thai, Malayalam and Hausa. The objective of the study was to find out whether in these languages the same morphemes were employed for designating both physical and psychological properties, and whether the psychological meanings of the terms in question agreed between them.

Thus, it was established that all the languages examined had morphemes describing both physical and psychological qualities. What is more, some of such physical-psychological pairings turned out to be identical with the English couplings, for instance, the coupling 'straight'- 'crooked'. The situation with other morphemes was slightly more difficult, as psychological meanings of some morphemes were not identical across the languages ('hot'- 'cold'). However, as Asch remarks, there was indisputable similarity between the meanings these morphemes have in different languages:

> "For example, the morpheme for 'hot'... stands for rage or wrath (Hebrew), enthusiasm (Chinese, Malayalam), sexual arousal (Thai), energy (Hausa), or nervousness (in Shilha, a Berber language, the phrase 'his head is hot' = he is nervous). Similarly, the morpheme for 'cold'... stands for self-possession (Hebrew), indifference or hostility (Chinese), loneliness (Thai), laziness or apathy (Hausa)." (p. 33)

Asch further makes an interesting suggestion that the variations in the psychological meanings of a morpheme might be due to the differences in their physical meanings. That is, as physical qualities have many aspects, any one of them may become the basis for a psychological sense of a term. He gives two examples of how the physical meaning of "colourful" influences its psychological meaning: in Homeric Greek, the psychological sense of "cunning" corresponds to the precise physical meaning of "changing the colour"; in Hebrew, the psychological sense of religious hypocrisy is paralleled by the precise physical meaning of being "painted over". And thus, Asch concludes his preliminary study of double-function adjectives across languages stating that there is no automatic agreement, but that the "growing" of a term in a psychological direction agrees with some generally valid principles. Therefore, the future task becomes to clarify the psychological conditions for the designation of physical and psychological properties by the same morphemes.

In 1955, Asch was not as yet prepared to provide a theoretical explanation of why a large number of terms in a language fulfils a dual referential function, noting only that most subjects themselves preferred the intrinsic rather than the associative interpretation, i.e., assumed that we in some respects experience other people as we experience physical objects rather than accepting that all such usages are conventional and results of historical accidents (or transfers of meaning). However, he refused to accept these naive accounts as a valid

explanation on the grounds that such introspective evidence is itself in need of explanation, since the same operations that had to be clarified were employed in subjects' reasoning. In an article of three years later, he discusses the same set of cross-linguistic data (with the addition of Burmese) and offers some ideas as to the conceptual basis of dual terms.

Thus, in 1958 Asch remarks again on the singular appropriateness of physical terms in descriptions of psychological qualities, observing in passing that there are hardly any exclusively psychological terms, and that the psychological meanings of dual terms in a context are apparently independent for the language speakers of their meaning in other contexts. Illustrating the cross-linguistic pairings this time on the morphemes for 'sweet', 'bitter' and 'sour', Asch stresses the "impressive agreement" among them which cannot simply be a matter of positive or negative psychological evaluation because the psychological meanings of these taste terms are much more differentiated than that – "sweet" never stands for courage or honesty, while "bitter" and "sour" do not designate overtly aggressive attitudes, etc. Continuing the line of thought Asch developed in his 1955 paper, one could see why it is not surprising that the psychological meanings of double-function adjectives are more than undifferentiated evaluative judgements: the same adjectives are relative terms when applied to the description of physical reality, picking out different aspects of a quality in different languages, and bearing an evaluative judgement only relative to the context of utterance. Similarly, in the psychological meanings of dual-function terms there is no inherent ethical judgement either, and even when there is one it is more often than not a function of the context (see also below)[56].

This later paper also contained a classified list of divergences found in the data. They included: a) cases of meaning restriction where a given morpheme denotes only a physical quality; b) different ranges of psychological meaning for a given term; and c) cases where it was difficult fully to establish the psychological sense of a term. I will return to these findings of Asch's in the next section. Presently, it will suffice to say that there were found no contradictory instances (such that the morpheme corresponding to 'straight' in its physical sense would correspond to 'crooked' in its psychological sense, for instance) and that differing meanings of a morpheme never turned out to be heterogeneous, but "specializations of a more general property in which they all share" (p. 90). As in a previous article, the same conclusion begged for acceptance: there is no one-to-one agreement, but the agreement there is seems to be law-governed and substantial.

---

[56] Even "straight" and "crooked" that seem to embody an ethical judgement can carry different evaluative attitudes in different contexts: "a bit crooked" may express admiration in a society prohibiting private initiative, and "straight" may be a negative evaluative judgement when uttered by a crook.

At that point it became clear that linguistic evidence alone would not be sufficient, and that a psychological analysis of the operations responsible for the observed agreement was needed. For Asch, there was no difficulty in explaining the presence of terms denoting both physical and psychological properties: no psychological continuity exists between the observer and the observed, other people become known through their "physical energies" (posture, tone of voice), and thus the existence of "physicalistic" terms should be no surprise. More problematic was the explication of particular physical and psychological associations.

The two ready interpretations of dual terms available at the time were an intrinsic interpretation, stating that we experience certain qualities in things and persons in some fundamentally similar ways, and an associative interpretation, claiming dual terms to be the results of stable associative connections between dissimilar conditions of physical and psychological stimulation. Asch rejects the associative interpretation, doubting that the association of a psychological event with a component of the corresponding physical event (to pale = to be frightened) works for a large part of dual terms (e.g., colourful). As to the intrinsic interpretation, or interpretation through stimulus similarity, Asch remarks that it is nearly impossible in most cases to find a stimulus condition shared by the physical and psychological settings which are described by the same term. Thus, dual terms had to be examined more closely.

Such closer examination suggested that in using terms like "hard", etc. one refers to one's phenomenal experience and describes the mode of interaction with the object in question (hardness = resistance to change). When applied to descriptions of people, dual terms also refer to a mode of interaction with a person in question. Despite the differences in content and complexity, the schema of interaction, according to Asch, is experienced as "dynamically similar". Thus, he contends:

> "The conclusion we draw, and one we consider essential to a solution of the present problem, is that the terms under discussion refer not alone to unique sensory qualities, but to functional properties or modes of interaction. They do not denote exclusively the 'raw materials' of experience; they are also the names of *concepts*." (p. 93)

What Asch means here is that the content of dual terms (such as *straightness* or *sharpness*) includes not only those aspects that can be established by psychophysics, but also "physiognomic" and dynamic properties which still have to be investigated[57]. And

[57] Where "physiognomic perception" means the internal, and perhaps innate, process of dynamic schematization of experience, or the understanding of the latter through the motor and affective attitudes of the perceiver. Billow (1977) in his review of psychological literature on metaphor,

concepts are understood as neither generalizations of different instances of experience, nor abstract logical operations, but concrete cognitive operations in terms of which people "naively comprehend events and similarities between them" (ibid.). Thus, the conclusion of the 1958 paper was that terms describing both physical and psychological events refer to their shared functional properties, although it is not entirely clear how Asch saw the relationship between them in what concerns their status with respect to each other. Given that, the aim for an empirical investigation became to study how the usages of double-function terms develop in children, and this is the next and last paper by Asch that I am going to deal with in this section.

Thus, Asch and Nerlove (1960) conducted a series of experiments on the comprehension of double-function adjectives by children of the following age groups: 3.1-4.11, 5.10-6.1, 7.6-8.0, 9.3-10.0 and 10.11-12.1. One of their primary objectives was to establish whether children were aware of the double usage and understood the connections between the meanings. The comprehension of the physical meanings of the terms *sweet, hard, cold, soft, bright, deep, warm* and *crooked* was tested on a number of objects displaying these properties, after which each child was asked whether these terms could describe persons. In the first group, *sweet* was the only term that was understood by the majority of children in its psychological meaning being generally taken to mean 'good' or 'nice'. For other terms, most of them in application to the description of persons provoked an indignant reaction, and in a few cases where they were accepted, they were taken to refer to physical properties of persons (*cold, warm, deep, bright*).

In the second age group, the physical sense of the terms was still dominant, although in a number of instances some of these terms were used to describe psychological properties (they included *sweet, soft, hard* and *bright*), and the explanations for the double usage were mostly given in affective terms (likeable – dislikeable). In the third age group, Asch and Nerlove observed a significant increase both in the use and the understanding of psychological meanings of double-function adjectives, although children in this age group had difficulty formulating the relationship between the physical and psychological meanings, sometimes suggesting that there was no similarity between the two ("They are two different kinds of *deep*" - see p. 53). Children in the fourth age group showed further

---

somewhat unjustifiably considered Asch's research in terms of the mediation hypothesis (natural correlations among experiences), while classifying Werner and Kaplan's (1963) as a nativistic theory of metaphor comprehension. It seems that all the way long Asch was quite uncertain on the issue of empiricism-nativism: he was prone to admit that the psychological meanings of double-function terms were later extensions of their physical meanings on the basis of directly experienced similarity, but at the same time understood that such an interpretation meets the problem of explaining the similarity in stimuli conditions.

increase in the comprehension of psychological meanings and also in their ability to find grounds for similarity. Finally, in the fifth age group there was a significant advance in comprehension and in the general adequacy of the explanations of dual function.

Such results were not expected by Asch and Nerlove as it turned out that the psychological senses of double-function adjectives become acquired as independent meanings, without any contact with the physical meanings of the terms. That is, it was unexpected that similarity played such a little role in the process of acquisition. Nonetheless, Asch and Nerlove were still insistent on the point that the similarity expressed in double-function terms was the similarity of experiences, and hence their warning:

> "The failure to use or understand a term is not evidence of failure to have the corresponding experience. It would indeed be strange to hold that children are insensitive to a person's properties such as *warm* or *cold* on the ground that they have not included them in their speech." (p. 56)

There were no simple conclusions drawn from these experiments: it was stipulated that the development of language in individuals does not need to follow the history of the language (thus, noticing similarity is not a necessary condition for the acquisition of psychological meanings of polysemous terms). A possible objection to this which stated that children were aware of the relation of meaning similarity but were unable to express it was rejected for the lack of evidence. And finally, it was proposed that there might be a "bridging procedure" operating between the stages of comprehension, where the transition is from physical-emotional to distinctly psychological usages (from "*warm* people make you feel warm" to "*warm* people are kind"). In sum, Asch and Nerlove's data showed a clear trend in the development of the understanding of double-function terms, but it did not give a clear indication of psychological conditions responsible for these changes, nor, for that matter, did it clarify the respective status of physical and psychological meanings.

4. 3. Discussing Asch's research: cross-linguistic study.

I have spent a long time in the previous section discussing research of almost half a century ago, but with good reason, because if one now leaves aside the ideas of physiognomic and dynamic perception, one will be amazed by how contemporary Asch's theory sounds. What is new in what I propose in my thesis amounts to the following: there is less polysemy than it seems; what appears polysemous to us is sometimes the result of our possessing a number of psychologically primitive concepts (innate, atomic and universal),

which account for the polysemy effect in certain terms (such as double-function adjectives) but which, nonetheless, are not sufficient to give the meanings of the terms. That is, what I am arguing for, is not an intrinsic "experiential" interpretation as suggested by Asch, but an intrinsic *conceptual* interpretation, where primitive concept possession precedes and organizes instances of experiences [58]. And thus, let us now return to Asch and consider his work from this perspective.

What I find particularly striking is that Asch (1958) actually spoke of double-function adjectives as being "names of concepts", where, for example, 'sharp' does not simply denote sensory qualities but more than that. For Asch, this was the mode of interaction with objects or their functional properties, that is, an analogical model of comprehension - sharpness as a psychological quality produces the same impression and requires a similar mode of interaction with it as sharpness in physical objects. For Asch, the concept 'sharp' is a concrete cognitive operation, in terms of which we *naively* comprehend phenomenological similarities (see p. 93). While agreeing that analogical reasoning is a source of many other polysemous terms (and also metaphors), I will later argue against an analogical treatment of polysemy in double-function adjectives and synesthetic expressions. For the moment, I suggest we take it for granted that concepts are not methods for sorting stimuli by the effects they have on us, but simple facts about our mind-to-world relation (i.e., it is not that you somehow acquire the concept 'deep' and later in life realize that deep feelings are similar to deep ponds, but rather that the possession of 'deep' is our biological endowment, and its application to a variety of experiences is a matter of what becomes stimulated in one's experience). The question becomes: how does the innateness of concepts such as those named by double-function terms agree with the divergences in the data on the psychological meanings of double-function adjectives?

First of all, recall that no contradictory psychological meanings were found across historically and physically remote languages discussed in Asch's study, and as noted by Asch himself, the differing instances produced an impression of being "specializations of a more general property in which they all share" (1958: 90). Consider some of these instances:

Cold:		Hot:	
	self- possession (Hebrew)		rage/wrath (Hebrew)
	indifference (Chinese)		enthusiasm (Chinese, Malayalam)
	hostility (Chinese)		sexual arousal (Thai)
	loneliness (Thai)		worry (Thai)
	laziness/apathy (Hausa)		energy (Hausa) (from Asch 1955)

[58] To steal an expression of Fodor's: we are good property-detecting mechanisms, which are tuned to *deepness*, etc. in all their instances.

Now consider the range of psychological meanings for the same morphemes in English:

Cold:	Hot:
lack of ardour	excitability
lack of warmth	intensity of feeling
no intensity of feeling	anger
lack of enthusiasm	sexual desire
lack of heartiness	feverishness
lack of zeal	violent exertion
indifference	suffering
apathy	intensity
unexcitability	violence
unimpassioness	rage
not flurried	
not hasty	
deliberateness	
lack of sexual passion	
lack of feelings	
cold-bloodedness	
lack of emotions	

(from OED[59])

Now to the points:

As is clear from the above list, the range of psychological meanings that English has for both 'cold' and 'hot' includes the psychological meanings of corresponding morphemes in the languages studied by Asch. That is, "cold" in English does not just mean 'unemotional' or 'apathetic' but has the whole range of related meanings. Similarly, being a native Russian speaker, I can vouch that Russian morphemes for 'cold' and 'hot' convey the same range of meanings without being restricted to any one of them. This suggests that other languages may also express the full range of meanings of "cold" and "hot" instead of being associated with just one psychological meaning. Some of Asch's examples show that this indeed may be the case: "cold" in Chinese was noticed to mean both 'hostility' and 'indifference'; "hot" in Thai means both 'worry' and 'sexual arousal'. Therefore, we can assume that 'cold' and 'hot' are both primitive concepts [60], and that the psychological meanings of "cold" and "hot" cannot be uniquely given by listing their synonyms in different contexts.

Similarly, if we consider more closely terms like "sweet", whose corresponding morphemes in languages other than English may express what seem to be differing psychological meanings, we shall discover that unification is possible. Below is the list Asch (1958) gave for the psychological meanings of "sweet"-morphemes in the languages he studied:

---

[59] I have deliberately broken OED entries into a larger number of psychological meanings to make clearer the points I am going to spell out below and to avoid the problem of explaining synonymy, i.e., reasons for classifying "cold-bloodedness" and "lack of emotions" as instances of one psychological aspect and "indifference" as an instance of another psychological aspect of the meaning of "cold", etc.

"Hebrew: sweet to the soul (said of pleasant words) (Prov. 16: 24)
Greek: sweet laughter, voice (etymologically linked with the verb of please')
Chinese: a sweet smile (colloquial); sweet, honeyed words = specious words
Thai: to be sweet is to faint; to be bitter is medicine = beware of people with whom you have relations
Hausa: I don't feel sweetness = I don't feel well
Burmese: face sweet = pleasant-faced; voice sweet = pleasant voice; speech sweet = pleasant speech" (p. 89)

At first sight, these instances may appear incompatible. However, as Asch remarks himself, the evidence for this statement was limited. By analogy to the previous examples, I would think that if more evidence were available from the languages in question, the incompatibility would have disappeared. The Russian language also possesses a "sweet"-morpheme which may be used to refer to some psychological reality. As it happens, both "sweet" as "pleasant" and "sweet" as "specious" are available depending on the context. When we are talking about the physical meaning of "sweet" ("not salt, sour or bitter" and "having the pleasant taste characteristic of sugar", as COED puts it), we may devise testing procedures for ascertaining whether something is sweet or not (be they objective or intersubjective). Nevertheless, this is a different matter from whether a given speaker judges something as "sweet-pleasant-to-the-taste" or as "sweet-but-a-bit-too-much". Similarly, we could probably devise methods for finding out what particular psychological features the speakers of a language label "sweet" or the like, which is a different matter from whether they think that to be a sweet person is a good thing or not [61].

Discussing the observed divergences in the data, Asch remarked in the same paper that one had better compare languages as systems rather than individual units of these languages. In those instances where a morpheme of a language denoted only a physical quality, it might have been the case that another term already met the need of expressing some aspect of the psychological reality, and thus the morpheme in question was not imbued with a psychological meaning. Similarly, in those instances where it was difficult to establish the

---

[60] How we get both physical and psychological meanings from the same primitive concept is a different question that I will take up later.

[61] In any case, I am not claiming that everything is given well in advance: 'sweet' may be a primitive concept, but not 'honeyed', which is a later utilization of 'sweet'. Similarly, the psychological meaning of 'cold' may be primitive and innate, giving rise to more sophisticated descriptions as "Arctic was coming out of his eyes".

Another possible objection to the existence of primitive concepts is that for some concepts it is more difficult to provide a "core" than for others. The single core for "hard" may well be something like "resisting change" that can be applied both physically and psychologically, but what about a single core for "sweet"? I suppose that an immediately workable paraphrase is indeed difficult to find. However, this does not entail that no core is available as a matter of principle: "eliciting a pleasurable response of a certain kind (perhaps subject to psychophysical investigation) from the experiencing subject" is a possible paraphrase. Note that sweetness is not an "objective" property (which could be something like "free of sulphur compounds"): sweet things are only such to representing systems that are able to represent them as 'sweet'.

precise psychological meaning expressed by a term (like the meaning of the morpheme corresponding to the English "spoiled" in Thai where *spoiled heart* is 'to be sad' and *heart spoiled* is 'to be discouraged' - see Asch 1958:90), it might have been the case that a fuller understanding of the syntactical properties of a language in relation to its semantics would have clarified how a particular aspect of meaning is chosen by the interpreter (note, however, that even in the above example there is some general property shared by both instances of "spoiled").

In his 1955 paper Asch touched on the physical meanings of double-function adjectives, suggesting that the slight differences in their psychological meanings across languages may have as their origin differences in physical meanings, i.e., aspects of the physical reality that are depicted by them may not be uniform across languages and cultures (see previous section for examples). While agreeing that the most salient (be it central or frequent) physical meaning of a term may influence the salience of its psychological meaning, I do not think that one should consider physical meanings (such as those of "hard") as not being uniform across languages (one will surely know whether something is hard for its own kind or not without necessarily knowing how to define the "degree" of hardness). As was shown for the psychological meanings of "cold" and "hot" in English, it can also be shown for the physical meanings of the corresponding morphemes in other languages that the range of physical aspects covered is uniform plus or minus a bit, and thus that the physical meanings of "hard" and the like are not culturally variable.

4. 3. 1. Notes on conceptual atomism.

At this point, I need to make a little warning: the concepts 'cold', 'hot', 'hard', etc. are atomic, which implies that to acquire the meaning of "hard", be it the physical or the psychological meaning, one does not need to previously possess or simultaneously acquire the meaning of "soft" or any other meaning. The idea of the meaning of a term as given by its relation to other terms in a language is especially prominent in semantic field theory (Kittay 1987, Lehrer and Kittay 1992), one particular version of meaning holism. The standard objection brought by philosophers against this approach is that the dependency of the meaning of a term on the meanings of other terms in the same field cannot preserve the referential relations of the language to the world since changing the meaning of one term in a field would imply changing the meanings of all terms in the field (see, for instance,

Hintikka and Sandu 1995)[62]. Apart from that, there is and would be, in the case of meaning change an irresolvable problem of the individuation of semantic fields for a vast majority of concepts (see Ludlow 1991).

I mention here the theory of semantic fields because culturally-oriented semantics/pragmatics theorists are likely to object to the idea of psychological meanings of double-function terms being manifestations of some innate, and hence universal, primitive concepts precisely on the grounds that meanings are determined by the relationships existing within a language as a whole, and that these relationships differ across languages. To give an example: Morgan (1993/1979) criticizing Searle's contentions about the cross-cultural validity of his examples (one of them being "cold": unresponsive people are cold), writes:

> "Out of curiosity, I checked with some friends from abroad on the figurative uses of Searle's examples and a few others. The consultants were Nepali, Arab, and Chinese. I checked for figurative use of 'blue', 'yellow', 'green', 'cold', 'warm', 'square', 'bad news', 'sunset', 'dawn', 'moon', 'sun', 'wolf', 'pig', 'fox' and 'dirt'. I found as many differences as similarities. None accepted 'warm' as a figurative counterpart of 'cold'. I was told that in Arabic, there are two words for 'cold', only one of which has Searle's figurative use. One could conclude from this that one has to *learn* that 'warm' is used figuratively to mean friendly, responsive, and so on." (p. 131)

However strange it sounds, Morgan's examples confirm the idea of conceptual atomism. Consider: the assumption that the meaning of "cold" is intrinsically related to the meanings of "warm", "hot", "cool" and any other terms that one happens to consider as relevant to the domain of thermal sensation ("scorching" or "freezing" would not be so bad for these purposes) leads to the idea that the absence of such additional terms in the vocabulary of some particular language means our inability to find out what an expression of this language means (should it be translated into English as "hot" or "cold" or something in between the two). Thus, we get the problem of radical interpretation.

Contrary to that, I hold that there are more similarities than differences between people of different languages and cultures, and this is due to our being members of the same species. Thus, we can take just one step further, and say that conceptual similarity is to be expected, that "hot", "cold" and the like mean precisely 'hot' and 'cold' across languages. As such, there is no need for them to be conceptually related: one does not need to know anything about cold things in order to be able to react in a proper way to hot things, and if

---

[62] Note that Hintikka and Sandu's argument against the semantic fields theory of meaning is of the same nature as the one I employ against Marks et al.'s putative theory of sensory meanings in the previous chapter (although "loud" is a relative term, perceived loudness is an objective quality, which is independent of the perception of quietness relative to a certain kind of stimuli).

one is a sophisticated enough creature (a human being), to have the concept 'hot' activated by these experiences.

Still, there may be three objections to the atomistic view. First is that "hot", "cold", "cool" and "warm" definitely are related. Second, that there are unlikely to be languages that have a word for "cold" but lack a word for "hot". Third, that people do use the conceptual relationship between "hot" and "cold" in reasoning.

Let's start with the first objection. It seems to be grounded in the view that since hotness, warmness and coldness are related as properties of things in the external world, they must be necessarily present and related in one's conceptual system.

Imagine now a hypothetical parasite (migrated from Dretske 1995: 82), that attaches to its host only when the latter has a body temperature of +18 C, that is, the temperature is registered to be +18 C by its temperature-detecting mechanisms (which we can establish by outside observation). Imagine also that we may endow our parasite with a minimal representational capacity and a minimal conceptual system (or language of thought), and assume that it is such that it is able to form part of a larger human conceptual system if expanded. Imagine finally that the only temperature-related concept the parasite has happens to be 'warm', which is connected with the temperature at which it attaches to its host. This way the parasite may represent the temperature of +18 C as the temperature of +18 C and also as something 'warm' (but since it is only responsive to the temperature of +18 C, 'warm' for it is exactly the temperature of +18 C). The temperature of the parasite's host may drop to –18 C (due to refrigeration) or raise to +58 C, reflecting changes in the external world, but that would not affect the parasite's representational abilities: it would still have the concept 'warm', but not 'cold' or 'hot' (although it may have the concept 'other-than-warm'). Thus, 'warm', 'hot' and 'cold' do not have to be conceptually related as a matter of principle.

Humans are a bit more complicated than hypothetical parasites. They have all of these concepts, and no language is known that would have a word for "cold" but lack a word for "hot" at the same time. Whether one takes coldness and hotness to be objective properties of things (as determined by thermometers and external reference frames) or relative intersubjective qualities (things are cold or hot because they appear as such to the sensory apparatus), it is obvious that humans' survival is affected by both poles of the continuum (you may freeze or you may burn, neither of which is a nice thing). Thus, there is nothing surprising in the fact that human languages should express both concepts. On the contrary, inhabitants of the planet K40, whose conceptual system is in may respects similar to ours, who cannot sense the 30 (but only 31) degree differences in temperature, where the average

temperature is 550 C, where the temperature never falls below 530 C, and where the only danger coming from the environment is severe temperature raises of 100 degrees or more, are most likely to have in their language a word for "hot", but not for "cold" (using instead a word for "OK" or something of this kind).

Again, humans are not K40ians and seem to rely on the conceptual relationship between 'hot' and 'cold' in their reasoning. I would not have said anything to the contrary. However, the fact that they do and the question of conceptual atomism are different issues. Things in the external world may possess all kinds of temperatures in the continuum of $-\infty$ C to $+\infty$ C, but it is the sensitivity of perceiving and representing systems that matters to what parts of the continuum are conceptualized. The fact that 'hot' and 'cold' are connected in human reasoning reflects the fact that humans are influenced by that difference in their activities. K40ians, for example, are more interested in the relationship between hotness and okayness.

Returning now to Morgan's example from Arabic, one can see that he did not provide sufficient evidence to show the physical usage of the two terms for 'cold'. And thus, it is somewhat difficult to accept his claim about the figurative uses. English has a number of terms - "spoiled", "rotten", "addled", etc. which mean pretty much the same thing, only that "addled" is normally predicated of eggs. Should we be surprised than children can be spoiled, but not addled? Or that tempers can be rotten as much as teeth can? Similarly, when Morgan says that the psychological meaning of "warm" has to be learnt independently, he means that it has to be learnt independently of the knowledge of what "cold" means. But this is the point I have just been making about conceptual atomism. What his example does not show is where the knowledge of the physical and psychological meanings of "warm" comes from, which is the point of the whole matter.

The psychological meanings of the terms in question generally exhibit even less necessary conceptual relatedness. Even though the vast majority of languages (as shown by Asch's study) have psychological meanings for both "hot" and "cold", it does not follow that the psychological meaning of one is conceptually necessary for an individual to know the psychological meaning of the other. To demonstrate this one can simply extend the argumentation concerning the physical meanings of those terms. Just one more example: when one knows what "a cold person" means, one knows what behaviour this kind of person is likely to exhibit in certain situations. This is a sufficient condition for understanding "He's a cold person" and for inferring that he might have reacted in such and such ways in the situation. But it is not part of a sufficient condition to know how a warm person, or a hot-tempered person would have behaved in the same situation. The relevance condition does not extend that far.

Thus, my point is that to know what "a warm welcome" means one is not required to know what either "a cold dinner" or "a cold reception" could be. However, one is required to know or be able to know what "a warm jumper" is. Similarly, the other way round: to know what "a warm jumper" is, one is required to know or be able to know what "a warm welcome" means, although one may be totally ignorant when it comes to "cold dinners"[63]. In short, the possession of a primitive concept requires that one be ideally able to recognize all of its instances[64], but it does not require that one possess any other concept (the fact that one does is a metaphysical, not a conceptual necessity).

To summarize. In the last two sections, I have argued for the innateness and atomicity of concepts expressed by double-function adjectives. I have shown that arguments from cultural relativity and examples intended to prove cultural variability in what I called primitive concepts are not conclusive to make one reject either the idea of conceptual innateness or the idea of conceptual atomism. In the next section, I will do the same to possible arguments from child development.

4. 4. Discussing Asch's research: language acquisition study.

As stated in section 4.2, Asch and Nerlove's (1960) research on children's understanding of double-function terms provided some evidence on the developmental timetables in the acquisition of psychological meanings, but practically no evidence on the role of similarity or the influence of physical meanings on their acquisition. Asch and Nerlove retained their position, according to which the inability to express features of experiences does not entail the lack of such experiences and suggested that a bridging procedure was operating between physical-emotional and psychological meanings, although this suggestion was mostly based on indirect observations rather than clear-cut evidence.

---

[63] A great deal of this may be perhaps tested empirically. For example, in second language acquisition studies. Subjects may be asked to guess at the psychological meaning of a term, whose physical meaning was explained to them, and at the physical meaning of a term, whose psychological meaning was explained to them. Provided that their knowledge of a foreign language is somewhat limited, as a next trial they may be asked to predict whether the antonym of the term in question would have both the physical and the psychological meanings and what they would be. In explaining tenses to my students I use the example: "I have been having a rough time lately". When asked the meaning of "rough", I start by saying that it could be predicated of a desk to say that it is (a Russian word follows). None of my students needed further explanations. It would be interesting to see how reliable the explanations from the psychological to the physical are compared with the explanations from the physical to the psychological.

[64] As pointed out by Pr. Timothy Williamson, the claim may sound too verificationist. If it is too hot and one burns instantly, one may not recognize this as being an instance of 'hot'. That is why the

The point I have been making so far in this chapter has a lot in common with the explanation of a third-age-grouper - "They are two different kinds of *deep*" (Asch and Nerlove 1960: 53). The research did not show that the acquisition of psychological meanings made any contact with the physical meanings, so why suppose a relationship where there is none? As a primitive concept, 'deep' depicts those aspects of reality that exhibit deepness, the concrete sphere of its application determines what particular meaning it is going to have. The fact that younger children reacted indignantly to the possible psychological meanings of those physical terms that they already knew is not in itself sufficient for grounding psychological meanings in physical meanings - there are developmental timetables for meaning acquisition ("schizophrenia" is not everyone's first word), and psychological reality may be a more difficult domain to grasp.

At this point it is appropriate to wonder about the relationship between meanings and concepts in different developmental stages. Thus, I have been arguing that one underlying concept corresponds to many different but related meanings of a word. In the preceding chapter I presented evidence that until a certain age there is a one-to-one correspondence between, for instance, the concept 'loud' and the different meanings of the word "loud". According to Marks et al. (1987) a child starts to distinguish between the different meanings when he becomes aware of linguistic conventions which govern the ascription of labels to sensory modalities, but until this stage is reached, "loud" just means 'loud' in the sense of a child's readiness to agree to a number of stimuli from various modalities being loud. The question now is: could it be the case that with respect to double-function adjectives there is also a single inclusive meaning of the word corresponding to the underlying concept, at least in the early stages of language acquisition? And if there is not a single inclusive meaning, how could the failure of one-to-one correspondence be treated besides appealing to developmental timetables?

It seems that "sweet" could be considered to have a single inclusive meaning - it is the most positive psychological term from a child's point of view which was generally correctly understood by children of the first age group, and whose meaning in that stage was somewhat close to the meaning of "pleasant" (seven of the ten children in this group conceded that something that is generally good or nice can be "sweet" – see Asch and Nerlove 1960: 50). However, it does not seem to be the case with other adjectives, since the first stage was also characterized by an indignant reaction to the experimenters' suggestion that physical adjectives can describe persons (where the second stage is the acceptance of

---

word "ideally" is introduced. If conscious thought processes could catch up in speed with automatic perceptual responses, the instance would have been recognized.

the two meanings as equally valid but unrelated, and the third stage is the ability to see them as related through analogical comparison). It is interesting to note that some explanations given by older children who were ready to acknowledge a similarity between the different meanings of a term employed other terms, also having a dual function, as in "*Hard* things and *hard* people are alike in that neither of them break" (ibid.). And such an explanation already presupposes the knowledge of the two (or more) meanings of "break", etc.

I have been unable to trace any research on children's understanding of double-function adjectives that could be directly compared with Asch and Nerlove's, but arguably, a different testing procedure, e.g., one comparing indirect comprehension, direct comprehension and spontaneous usage, might have produced clearer results. It might have shown, for instance, if there were other terms than "sweet" which in the early stage exhibited the tendency to single inclusive meaning. This alternative may also be made available if one draws parallels between the understanding of double-function adjectives and the understanding of synesthetic adjectives, and uses such parallels them in the application of linguistic stimuli ("deep pond" – "deep colour" – "deep person"). In the absence of any other experimental results it is difficult to judge whether one-to-one correspondence is indeed present in the earliest stages. But here are some considerations as to how this question could be tackled.

The disagreement between children's spontaneous metaphoric speech and experimental research showing that children cannot appreciate the dual meanings of terms was investigated by Gardner (1974), where he came to the conclusion that the application of descriptive terms across unusual domains does not present a serious difficulty for children (his stimuli included *cold/warm, hard/soft, happy/sad, loud/quiet, light/dark* as applied to visual-colour, visual-physiognomic, visual-abstract, auditory, tactile and verbal-kinesthetic modalities), and that "the essential capacity for metaphoric association retains the same pattern throughout development" (p. 89; the youngest group tested were children of 3 and a half years old). In Gardner's study the physical-psychological opposition can be detected in the task of applying the aforementioned stimuli to the visual-physiognomic modality (stimuli application to Frois-Wittman faces). The percentage of correct matches made by pre-schoolers on that task was 67, being exceeded only by verbal-kinesthetic matches (76 % correct answers). This can be taken as evidence to the effect that even very young children are capable of associating personalities (through facial expressions) with what seem to be adjectives for physical reality. Among possible candidates are "hard" and "soft" on which there was the least number of mismatches across ages and which were easy even for the youngest subjects (p.88). This finding partially contradicts Asch and Nerlove's observations

for the same age group[65], and therefore leaves it open for speculation whether there can be other than "sweet" one-to-one correspondences with respect to double-function adjectives.

There may also be a good reason behind the observed discrepancy between children's ability to understand the word correctly and to give weak reasons for its being understood in this particular way. Vosniadou et al. (1984), although studying different sets of examples of analogical reasoning, interpreted their data as showing that successes or failures of metaphoric (alternative meaning) comprehension are the result of numerous factors rather than simply of a linguistic input, and further added that "inadequate paraphrases and explanations cannot be taken as evidence of comprehension failure" (p.1589).

Be that as it may, my claim has been that double-function adjectives are not metaphorical in their psychological meanings, as these psychological meanings are not derived from the physical meanings, but that both are manifestations of some primitive concepts which are mental manifestations of our ability to have certain experiences. This, I believe is true of many concepts expressing properties of things. A very interesting piece of evidence from child development research that seems to be pointing in the direction of innateness comes from an abstract by Curtis (1985), which I am quoting below in full:

> "The language learner encounters speech containing both conventional and novel word extensions. Through a sentence classification task, it has been found that young children (3.5 to 5.0 years) tend to regard sentences containing polysemous terms e.g. NECK (of a bottle), ARM (of a chair) as nonsense. Their ability to understand sentences containing such phrases increases with age. However, intelligibility/acceptance of sentences containing novel extended meanings based on the same principles, e.g. 'the legs of the scissors', 'the arm of the saucepan' remains low throughout the age group studied (3.5 to 7.5 years). This suggests (a) that familiarity is an important factor in comprehension of the polysemous terms used, (b) that the extended meanings of these terms are probably learnt individually rather than by reference to a general rule and (c) that on initial encounter with a familiar term used in a novel way young children tend not to make the inference necessary for comprehension."

What I like about this study, somewhat similar to that of Asch and Nerlove, is that it provides a contra-argument for the idea of metaphorical structuring of concepts - analogical or metaphorical reasoning may not operate at the early stages as a mechanism of concept formation (note that this is also a Piagetian view on analogical reasoning; see chapter 1). Certainly, familiarity plays a decisive role in the task of deciding between acceptable and unacceptable expressions, but the results can also be interpreted in favour of concept innateness. Naturally, no one is going to suggest that the English language should have

[65] It is perhaps remarkable that although "hard" was among the physical stimuli in Asch and Nerlove's experiments with the first age group, its acceptability as a psychological description is not reported (unlike that of "sweet", "cold", "deep", "warm" and "bright" – see 1960: 50)

inevitably come to express certain concepts in an analogical mode, while not expressing others in the same way. Nonetheless, ontogeny might and must be different from phylogeny - the history of meaning creation becomes the genetic endowment of later generations, thus rendering certain mappings more comprehensible than others[66].

Nevertheless, I am arguing that the case of double-function adjectives is slightly different from the polysemous nouns in Curtis' study. On the basis of their research Asch and Nerlove concluded that in double-function adjectives individual development departed from the historical development of language (p. 60): first because children did not use the support of physical meanings in their acquisition of psychological meanings, and second because the development of human languages had to be analogical - moving from the physical to the non-physical. I will discuss the analogical model of the development of languages later; for now it will suffice to remark that the analogy between the human body and other objects having protruding parts is easily traceable due to perceptual and kinesthetic schematism, while the stipulated analogy between properties of inanimate objects and properties of people's personalities and interpersonal behaviour is less clear, especially as it is not even clear how properties of different objects become properties known to us by the same name (see previous section).

In short, the issues in children's acquisition of double-function adjectives cannot be considered as an argument against concept innateness, the major reason for this being the absence of any reliable connection between the two meanings in the process of acquisition itself (setting aside later explanations of the meanings of terms).

---

[66] I do not contend that this is the only viable explanation. However, if there is indeed anything like the language organ, selective responsiveness to some noun-noun combinations may be pre-specified in the Darwinian sense of adaptability and canalization. If an analogy is helpful here, then one could mention Storfer's (1999) research on causal links between myopia and the surge of intelligence.

Chapter 5. Evidence from double-function terms (II).

5. 1. Polysemous adjectives from the point of view of psycholinguistics.

5. 1. 1. Williams' (1992) study.

Double-function adjectives have not been the central object of psycholinguistic research. The only work I managed to trace that is directly concerned with the issue in question is Williams (1992), which I discuss in some detail below.

Williams (1992) conducted two experiments examining the meanings of polysemous adjectives that have both physical and psychological meanings, such as "firm". One of them used a priming technique, the other a relatedness judgement task. The purpose of the experiments was to establish whether the meanings of such polysemous terms are independent of each other in the mental lexicon as the meanings of homonyms are (e.g., Tabossi, Colombo and Job 1987), or whether they are interrelated and exhibit hierarchical structuring within a smaller number of lexical entries. That is, in priming experiments all meanings of a homonym become "initially activated independently of the context" (Williams 1992: 196), although the context immediately works to select the appropriate meaning. Thus, if polysemes are mentally represented in a way similar to that in which homonyms are represented, then all their various meanings should also become initially active in priming tasks.

As materials experiment 1 used the following set of adjectives: *awkward* ('clumsy' vs. 'embarrassing'), *tight* ('taut' vs. 'compact'), *dull* ('dreary' vs. 'stupid'), *firm* ('solid' vs. 'strict'), *smooth* ('even' vs. 'slick'), *deep* ('low' vs. 'profound'), *dirty* ('soiled' vs. 'obscene'), and *strong* ('mighty' vs. 'intense') which were tested in a single word priming task and then compared with priming from the same words in sentence contexts at delays of 250, 500 and 850 msec. Experiment 1 showed that in isolation the two readings of a polysemous adjective (e.g., 'strict' vs. 'solid') are equally primed for by that adjective (*firm*). However, in sentence context, the results were asymmetrical between central ('solid') and non-central meanings ('strict'). In those cases where the context suggested the central reading, the non-central targets were not primed for. By contrast, where the context suggested the non-central reading, the central targets were primed for (even at delays of 850

Although myopia is primarily an inherited condition, its prevalence is affected by the rise in intelligence and changes in the brain size (the human brain growing "intergenerationally").

msec after offset). Although there was also some degree of priming in the non-central conditions, it was unstable over items and subjects (Williams 1992: 201).

Thus, the results of the priming task experiment show that the irrelevant meanings of a polysemous adjective do not become suppressed as the irrelevant meanings of a homonym do. This allowed Williams to hypothesize that the meanings of polysemous terms are neither independent in language comprehension nor equal in their representation in the mental lexicon, but interrelated in some important ways such that some of them persist even in the irrelevant contexts.

Experiment 2 employed a direct relatedness judgement task, where subjects were asked to judge the relatedness of target words to sentences, being tested against related and unrelated pairs of central and non-central targets. It was performed in order to articulate more clearly the difference between central and non-central meanings, as well as to receive a clearer indication of individual differences. After seeing a sentence on a screen and acknowledging that they had finished with it, the subjects were required to press either *yes* or *no* button in response to a target depending on whether they judged it to be related "in a general way" to the overall meaning of the sentence. Sets of testing materials consisted of four sentences with the following order of conditions – central related, central unrelated, noncentral related, noncentral unrelated – as in the following example (see p. 216):

"*Firm*
The headmaster decided that the boy would do better with a firm teacher. SOLID
The headmaster decided that the boy would do better with a young teacher. SOLID
The woman did not like her hotel room because it had a firm bed. STRICT
The woman did not like her hotel room because it had an unmade bed. STRICT"

Experiment 2 confirmed the results of Experiment 1 in that non-central meanings stop interfering with the subjects' responses at long delays and that the interference of the targets with the sentences overall meanings was confined to those cases where the central target followed the non-central uses of a polysemous adjective. That is, "solid" was more likely to interfere (interference with *no* responses or the increase of *yes* responses) with the overall meaning of the first sentence in the example cited, rather than "strict" with the overall meaning of the third sentence. Still, the results of the second experiment showed a higher degree of item and subject variability, exhibiting a significant interaction between centrality and relatedness for the subjects but not for the items[67].

Thus, the most general result of Williams' (1992) study is the persistence of the contextually irrelevant meanings of polysemous adjectives even at sufficiently long delays,

[67] There was even one example where the noncentral target produced 59% of *yes* responses. It was: "The engineer explained to his apprentice how to make a very smooth surface. – SLICK" (p. 216)

which seems to demonstrate that unlike the meanings of homonyms, the meanings of polysemes are interrelated, which is exactly how the difference between homonymy and polysemy is usually defined.

5. 1. 2. Williams on meaning hierarchies.

In the same paper Williams considers the cognitive linguistics idea of meaning interrelatedness in polysemous words, which claims the existence of "meaning chains" (the relationship between meanings is determined through their respective similarity to a set of meaning components), and rejects it on the grounds that this hypothesis cannot account for the difference in the degree of priming between the senses of a polysemous adjective. He also considers two other plausible explanations for the asymmetry, one articulated in terms of the relative frequency of the two meanings, and the other in terms of structural aspects of the meaning representation (see Langacker 1988). The latter assumes that central or prototypical uses (central nodes) get extended either by generalization ("upward"), specification ("downward"), or else metaphorically (from running as a 'rapid four-legged locomotion' to 'competitive political activity" – Williams 1992: 209).

As Williams correctly remarks, such a hierarchy does not explain how the various meanings are accessed during sentence comprehension. Therefore, the two facts that require explanation are the following: a) the presence of asymmetry in the activation process between the central and non-central meanings of a term, and b) the absence of perfect asymmetry in the priming of the central and non-central targets (the effect of the latter was noticeable at short, though not at long delays).

According to Williams, the lexical decision task involves three stages: recognition, response selection and, finally, response, which is preceded by a check on the relationship between the target and the context (p. 209), contributing either to the facilitation of lexical decision in those cases where a relationship is established, or otherwise to its slowing down. Thus, the fact that in the single word experiments there were no differences in the computing of the prime-target (central vs. non-central) relationship is explained as an instance in which the target itself "licenses the extension of the prime's meaning where necessary" (p. 210), without its being inconsistent with a hierarchical meaning structure.

However, with the introduction of a biasing context the computing of the relationship between a central target and the context does not become more complicated unlike the

computing of the relationship between a non-central target and the context[68]. The presumption is that in the former situations "the extension of the prime's meaning" is not incompatible with the context (see ibid.) because this extended meaning may be already implicit within that context and, in force of the hierarchical concept structure, explored during the activation process. This is not the case for the non-central targets which are only computed in felicitous contexts (no computation of the relationship between the non-central target and the context is performed in time for the response, and thus "downward" extensions of a prime are blocked by the context). The fact that on certain trials the computing of the relationship between the prime and the non-central target was successfully accomplished[69] is considered insignificant by Williams on the grounds that subjects have a tendency to overinterpretation.

From these results Williams concluded that the meanings of a polysemous adjective cannot but be interrelated and interdependent. Some meanings (central meanings) are privileged because of either their frequency distribution or else their position in the hierarchical meaning structure. The results were compared with other studies on polysemy and were found to be in agreement with those: Durkin and Manning (1989) showed that the importance of the dominant meaning of a word in a subordinate meaning context rates higher than the importance of the subordinate meaning in a dominant meaning context.

However, as Williams admitted, the results were not conclusive as to whether the centrality of a meaning of a term was determined by meaning frequency or by structural aspects of conceptual organization (see p. 212). Thus, indecisive between "ecological" (e.g., connectionism, which bases conceptual organization on associations and frequency conditions) and "intellectual" (e.g., cognitive linguistics, which bases conceptual organization on primary physical meanings and their metaphorical extensions) theories of concept formation, he left the whole discussion suggesting a need for future studies which might show whether the activation of certain meanings in irrelevant contexts would coincide with the activation of the most frequent meanings or not.

---

[68] Central target: "Everyone thought that he should become a salesman because he was so smooth" – EVEN. Non-central target: "The builder worked on the wall until it was quite smooth" – SLICK.

[69] One is reminded that Williams' experiments demonstrated high degrees of subject variability (see examples in the next section).

5. 1. 3. Discussing Williams' results.

Although avoiding a direct statement of preference for the role of the hierarchical meaning structure over meaning frequency with respect to the activation of concepts in infelicitous contexts, Williams, or that is the impression, is inclined towards it. And thus, he tends to explain the differential effects in priming for the two meanings of the polysemous adjectives via their being organized in a hierarchy with central (physical) meanings dominant over non-central (psychological) meanings. The impression one naturally gets is that this should show that the psychological meaning of an adjective is secondary and derived by some kind of meaning extension from its primary physical meaning. This is confirmed by Williams' observation that central meanings were implicit in contexts biased towards non-central meanings, and also by the choice of experimental materials, where the centrality of meanings was established with reference to their concreteness as well as their frequency. Etymologically, all but two adjectives ("dull" and "tight") had earlier physical meanings.

Now, to the question that interests me most in this context – the nature of concepts and conceptual representation. According to Williams, the results of the experiments suggest that the meanings of polysemous adjectives (and hence, corresponding to them, concepts or mental representations) are organized in such a way that the physical meanings are central in the hierarchy and extensible to the psychological meanings which are considered their derivatives. Such organization should explain the differences in the priming effects. However, the two experiments did not show the existence of such organization conclusively. In some instances, owing to both subject and item variability, the priming effects from non-central targets were equally significant. This was also noted in Kilgariff (1997), where he discusses psycholinguistic experiments as a possible means of establishing word senses:

> "the method would never be practical for determining the numbers of senses for a substantial number of words. The results of the experiments are just not sufficiently stable: as Williams says, the priming task 'suffers from a large degree of item and subject variability'." (p. 99)

What I intend to show in this section is that a "no polysemy" interpretation assuming the existence of primitive concepts which are "indifferent" to the physical-psychological differentiation of meanings is compatible with the results obtained by Williams in his study. My intention here, as everywhere in this chapter, is to show the in principle possibility of the view I am proposing. It will be the business of the next two chapters to show why this view may be preferable to others. However, before proceeding further in this discussion, let

us consider more closely the experimental material. Here is one of the examples of the situation where the context biased the non-central meaning towards the central target: "Everyone thought that he should become a salesman because he was so smooth – EVEN". In this example 'even' is considered as implicitly active within the representation of "smooth". The contrasting example is "The builder worked hard until the wall was quite smooth – SLICK", where the computation of the relationship was blocked by the context in a large number of trials.

If we consider the two targets with respect to their own meanings, we shall see that "even", apart from its physical senses, also has a psychological sense close to that of "smooth". And "slick" has a physical meaning compatible with the interpretation of "smooth" in the context of "smooth wall". According to COED, "even" can mean "equable, calm" when said of a person's temper, and this interpretation bears no conflict relation to the "salesman" context, even if the sentence/ speaker meaning becomes slightly changed. According to the same COED, "slick" can be rendered as "sleek, smooth" or "slippery", which are also quite felicitous in the "wall" context. It is no accident that the latter was one of those examples where the non-central target produced a large number of *yes* responses (59%). But what about other materials from the same study where the non-central target did not produce much agreement? Let us consider some of them.

1) "The first hour of the party was rather awkward. CLUMSY" vs. "The gymnast's movements were judged as rather too awkward. EMBARRASSING" "Clumsy" means "awkward in movement or shape, ungainly", and if we substitute "ungainly" for "awkward" in the first example, the desired sense will be achieved. "Embarrassing" means "causing (a person) to feel awkward or self-conscious or ashamed", and this meaning is overruled by the general meaning of the sentence (someone else, not the gymnast himself, judged the movements awkward).
2) "The schoolteacher was criticised for not being firm. SOLID" vs. "The couple wanted a bed that was firm. STRICT" "Solid", among other things, can mean "staunch and dependable", although this definition, clearly, does not cover the whole variety of psychological contexts in which the word "firm" may be found. "Strict" is 1) "precisely limited or defined; without exception or deviation" and 2) "requiring complete compliance or exact, reinforced rigidity", none of which goes well in the "bed" context.
3) "She was difficult to get along with because her moods were so strong. MIGHTY" vs. "He always helped his friends move house because he was so strong. INTENSE" "Mighty" means "powerful or strong, in body, mind, or influence", or colloquially –

"great, considerable", and these meanings are in no contradiction with the context of "moods". "Intense" means 1) "(of a quality, feeling, etc.) existing in a high degree, extreme, forceful"; 2a) "(of a person) feeling, or apt to feel, strong emotion"; 2b) "expressing strong emotion"; 3) (of a colour) very strong or deep"; 4) "(of an action, etc.) highly concentrated". None of these senses expresses the physical ability required in moving houses – the search for a possible relationship is banned by the context.

As we remember, Williams' contention is that the mental representations corresponding to the polysemous adjectives in question are structured hierarchically in such a way that, for example, we have physical strength in the central node for 'strong', and then, as an additional but related node for those instances of "strong" where the physical interpretation would be undesirable. This second node, supposedly, should have appeared as a result of a metaphorical extension (perhaps, as a result of a historical meaning extension). In the actual processing of polysemous adjectives, the central node gets accessed first and persists, even if the context requires further movement to the additional node. Thus, the differences in priming effects follow directly from the structure of conceptual organization.

In the next chapter I will doubt the applicability of etymological explanations to the issues in synchronic semantics and, hence, to the questions of psychological processing of polysemous linguistic items. For the moment it will suffice to remark that Williams' idea of conceptual organization is an idealized version of "what would have been there" barred instability of priming effects, high degrees of item variability and the so-called tendency to overinterpretation noticed in the subjects. Consider now the possibility where "strong" does not mean "physically strong plus psychological expansions", but rather... 'strong', that is, having this particular action/effect on the objects it comes in touch with.

In Williams' experimental materials, "mighty" had a better chance of persisting over long delays in the interpretation of the sentence "She was difficult to get along with because her moods were so strong" when compared with the persistence of "intense" in the interpretation of the overall meaning of "He always helped his friends move house because he was so strong". As mentioned above, the meaning of "mighty" has all sorts of overlaps with the meaning of "strong" to make it acceptable in the interpretation of the first sentence. "She was difficult to get along with because her moods were so mighty" is a sentence that would not cause too many difficulties of interpretation (even if it may sound strange or unconventional at first, intersubjective agreement on the best interpretation is likely to be quite high).

On the other hand, in all of its meanings as stated by COED, "intense" conveys the idea of a "high concentration", which may refer to a narrower set of physical application of force when compared with "strong". And it is much harder to agree on an interpretation for "He always helped his friends move house because he was so intense". Some speakers may find it unacceptable (his being intense cannot contribute to his ability to lift things), others may use all sources they can (to borrow Williams' way of putting it), and come up with an interpretation, or an explanation why being intense is a useful quality when moving houses (perhaps "intense" becomes overinterpreted as "active", etc.). Thus, this set of examples cannot be used to prove the hierarchical meaning structure with the physical meaning of "strong" in the centre of it, and the psychological meaning of "strong" at the edges.[70]

Now, if mental representations of polysemous adjectives are conceived of in the schematic way of having such and such effect upon those who come across them (strong suspicions when they come your way are as unpleasant as strong fists when they come your way), we won't have to say that the primary meaning of "strong" is 'mighty' rather than 'intense' (which would not be confirmed by single word priming experiments). We would say that "strong" has its meaning concretized depending on the items that it is being combined with in a particular context. The examples discussed above are the clearest examples of the strictly delineated physical and psychological meanings of a term ("strong" as 'mighty' and 'intense'), and one can see how the substitution by near-synonyms is made possible in some contexts and is prohibited in other contexts. In the other examples from this study (see above and also Williams' (1992) article), these differences are less clear ("even" as 'slick' and 'smooth'), and this may account for the tendency of subjects to establish relationships between what was the non-central targets and the context.

In sum, nothing in the outcome of Williams' experiments contradicts the idea that there are underlying primitive concepts (like 'strong') which cover equally physical and non-physical meanings of the terms discussed, without there being central and peripheral meanings, where peripheral meanings are understood as metaphorical extensions of the central meaning. What is more, this view has the advantage that if the representations of

[70] Indeed, there are too many meanings of "strong", even physical meanings, to decide which should be placed in the centre. "Strong material" and "strong hands" both seem straightforwardly literal. Clearly, "strong hands" are not strong because they are made of strong materials, and "strong materials" are not strong because they are good for lifting things (although they may be good for us to use them in devices for lifting things). However, they are both literally strong. But why then should "strong lights" or "strong smells" or else "strong suspicions" be considered metaphorical only on the premise that they are not made of strong materials or are not good for lifting things?

polysemous adjectives are taken as schematic rather than physically-centred, the tendency to overinterpretation and high item variability no longer seem surprising[71].

5. 2. Processing of metaphors and polysemes by brain-damaged subjects.

5. 2. 1. Research by Winner and Gardner (1977).

Inspired, in a sense, by the work of Asch on double-function terms and the work of Williams (1976) on synesthetic adjectives, Winner and Gardner (1977) sought to establish how the division of linguistic labour between the two cerebral hemispheres relates to the understanding of "connotative" aspects of language and also to shed some light on how this aspect of language is processed in normal conditions.

The designed experiment included matching 18 metaphorical phrases (9 double-function of the type "heavy heart" and 9 cross-sensory metaphors of the type "colourful music") with sets of 4 coloured pictures (representing the appropriate meaning of a metaphoric sentence, a literal representation, a salient quality described by the adjective, and the noun), which was followed by verbal explication of a metaphor. These tasks were administered to groups of normal and brain-damaged patients (with damages to the left hemisphere, with damages to the right hemisphere and patients with dementia). Their performance was evaluated according to the overall competence on each task and the relationship between performances on the two tasks in different groups of subjects with special attention paid to establishing profiles of errors where possible.

The results obtained showed that of all subjects a) normal adults chose more metaphoric pictures than any other group; b) individuals with left hemisphere damage chose slightly fewer metaphoric pictures; c) individuals with right hemisphere damage and dementia chose significantly fewer metaphoric pictures than normal adults. Considered on the grounds of

[71] I have been making quite a point out of the tendency to overinterpretation in connection with the Williams' study that I might have appeared to be stretching its importance. One brief mention may show that this problem is not something merely accidental to psychological research, but that it may bear strongly on the problems of language psychology. At a RAAM III session, J. Hoorn (1999) who tested among others the anomaly theory of metaphor (for more details on the view see Hoorn 1997; see also Martin and Harre 1982: 98) expressed surprise at subjects' tendency to interpret anomalies as metaphors, further suggesting a model where anomaly is a result of failed literal and/ or figurative interpretations. I am sure one could find more instances where the "unreliability" of subject's interpretations was considered as an impediment to getting clear results. However, if the question of overinterpretation arises persistently in experimental work then, perhaps, it should be considered as an indication of what is going on in subjects' heads during their understanding of words and sentences of a language, not as a matter to be disregarded.

their characteristic reactions (see p.723), a) individuals with right hemisphere damage chose as many literal as metaphoric pictures and, unlike other groups, they did not find the literal pictures strange or absurd; b) individuals with left anterior lesions chose a significant number of metaphoric pictures (67%); c) individuals with left posterior lesions chose 46% of metaphoric and 24% of literal pictures (the remainder were only-noun and only-adjective interpretations); d) individuals with dementia made an equal number of metaphoric and literal choices; and finally e) normal controls chose metaphoric pictures 73% of the time and were ready to correct their choices when asked to do so.

As to adjective and noun pictures, those were never chosen by normal controls, rarely chosen by individuals with dementia, and sometimes chosen by right and left hemisphere damaged individuals, although such choices were not made more than 13% of the time. When the patients were asked to give verbal explications of their choices, an interesting pattern of responses was discovered. Patients with left hemisphere damage performed poorly on the task, repeating the words in the sentences, while patients with right hemisphere damage were able to explain metaphors in general non-metaphoric terms. There was some interesting dissociation between the pictorial choices made by right-hemisphere damaged individuals and their verbal explications, which, however, did not disturb the patients themselves. The following is a very good illustration of this, upon which I will be drawing later, and thus I reproduce below a passage from Winner & Gardner (1977: 724):

> "Four of these patients, however, initially resisted the sentences as meaningless and then proceeded to interpret them appropriately. Thus, upon hearing a sentence containing the expression 'bright smell', one patient said: 'The word bright is in the wrong place. I wouldn't say bright smell, though it could be a *nice* smell.' Another, when asked to paraphrase a sentence containing the clause 'a dark song', said adamantly that he had never heard of describing a song as 'dark'; but he then readily paraphrased it as meaning 'slow and dreary'... Thus, although metaphoric expressions *sounded* wrong to these subjects, their linguistic comprehension remained relatively unimpaired. A dissociation seemed to exist, in brief, between what they *thought* they knew, and what *in fact* was known."

From the results I have restated above, Winner and Gardner (1977) concluded that the dominance of the left hemisphere for the denotative aspects of language, and the dominance of the right hemisphere for the connotative aspects of language were confirmed and clarified, although the interaction of the two hemispheres is required for a fully adequate performance in linguistic tasks involving figurative language. The performances of anterior aphasics (able to comprehend "semantically atypical uses"), the performances of posterior aphasics (showing the poorer over-all performance), and the performances of patients with dementia were in tune with what had been expected. The unexpected finding was the

dissociation between metaphoric verbal explications and literal picture selection in right hemisphere patients.

In the absence of any other reliable evidence (visuo-spatial deficits, etc.) and taking into account the general inappropriateness of responses and emotional reactions of right-hemisphere damaged patients, the authors suggested that it might be best to consider their performances as a "qualitatively different mode of metaphor appreciation". Although the authors doubted the strict distinction of the functions of the two hemispheres with respect to figurative language processing, much of such differentiation is found in later research, as we shall see below. What has to be kept in mind from this section is that the right-hemisphere damaged patients in Winner and Gardner's study manifested not the lack of a linguistic capacity in actually understanding polysemous terms, but rather the lack of an ability to provide appropriate responses to a stimulus and deficits of retrieval. Another interesting fact is that no category of patients showed any difference in the processing of synesthetic expressions and psychological-physical metaphors.

5. 2. 2. Later research on the processing of polysemes in brain-damaged subjects.

Since the research by Winner and Gardner (1977), it has been assumed by some researchers (see, for instance, Chiarello 1988) that while the left hemisphere is responsible for literal linguistic processing, the right hemisphere has much responsibility for the processing of figurative language. If it is indeed the fact that patients with right-hemisphere brain injuries have difficulty in understanding secondary meanings of terms (psychological meanings of those terms that have physical meanings and "incorrect" applications of terms across sensory domains), then in the context of the present discussion this would imply that, psychologically, physical meanings are primary and all other meanings are their metaphorical derivatives. For all that many people would be happy to agree with that point of view, it will leave many unresolved issues, one of them being the question of cross-sensory transfers. Thus, in this section I would like to say something more on the research into brain-damaged patients' linguistic abilities before taking up theoretical matters in the next chapter.

Brownell, Potter, Michelow and Gardner (1984) conducted a study (by the method of triads) on sensitivity to lexical denotation and connotation in unilaterally left- and right-hemisphere damaged patients (pairing of physical-psychological adjectival stimuli) which showed a "selective insensitivity" of right-hemisphere damaged patients to connotative

aspects of meaning with preserved sensitivity to denotation. As normal individuals demonstrate flexibility in their application of both denotative and connotative aspects of meaning under appropriate circumstances, the point of the study was to ascertain whether these two aspects are dissociable, i.e. whether they involve different mechanisms or different lexical representations.

The results obtained showed that right-hemisphere damaged patients avoided pairing adjectives on the basis of their connotative meanings, thus disclosing their difficulty with connotation even at the single word level (the difficulty right-hemisphere damaged patients have with processing bigger linguistic units containing non-literal language had been documented before – see p. 256). Aphasic patients, on the contrary based their judgements on metaphor and polarity ignoring denotative aspects, such as antonymy. Normal controls were more flexible in their choices, making use of both denotation and connotation.

These results seemed to confirm the idea that "the right hemisphere has as its particular province sensitivity to connotative aspects of meaning" (p. 256). Thus they confirmed the dissociability of denotation and connotation with respect to physical-psychological meanings of a term. With respect to normal individuals' knowledge of the meanings of polysemes this implied that

> "a normal individual's knowledge of word meaning is in reality a sum of separable lexical stores that are (1) mediated by different parts of the brain, and (2) devoted to different orders of semantic information." (p. 263)

Taking into account the tendency to literalness among right-hemisphere damaged patients together with their inadequate emotional and aesthetic responses, and inability to draw inferences from the context (see ibid. for references), one would be bound to conclude to the psychological primacy of physical meanings and the secondary status of the psychological meanings of physical terms. However, the difficulty that left-hemisphere damaged patients experienced with denotation suggests that the story may not be so simple. Brownell et al. (1984) speculated that owing to their impaired access to some aspects of denotation aphasic patients had to rely on more general conceptual or interpretive cognitive capacities mediated at least partly by the right hemisphere. Nevertheless, nothing in their experimental setting allows one to draw the line between the processing of linguistic information and the coming into play of elements of general knowledge.

The experiment was designed to test sensitivity to connotation at the single word level. Suppose that there is in the brain a concept 'deep', which corresponds to the word "deep". If RHD patients are more prone to pair "deep" with "shallow", aphasic patients to pair it with "wise", and normal controls exhibit the ability to produce both pairings, this alone will not

show that aphasic patients, unlike RHD patients, bring their general knowledge into play unless one had already assumed the primacy of physical meanings.

In a later paper, Brownell, Simpson, Bihrle, Potter and Gardner (1990) addressed the same question of the processing of alternative word meanings in RHD and LHD subjects using a sorting task, where targets were adjectives having metaphoric alternative meanings and nouns having non-metaphoric alternative meanings. This study was undertaken in order to test whether the metaphor deficit in RHD subjects is restricted to metaphoric processing or whether it reflects the inability of this group of patients to access secondary word meanings generally. The task included picking out two words that were most similar in meaning, each group containing a target, a synonym and a foil (for example, *deep – wise – lake*).

The results of this experiment showed that RHD subjects performed worse than LHD subjects on both conditions (metaphoric adjectives and non-metaphoric nouns). However, when correct responses for groups of subjects were compared, it became clear that there was a significant difference in the RHD subjects' appreciation of metaphoric alternative meanings and non-metaphoric alternative meanings, which was not the case with the LHD subjects. These results suggested that there is a qualitative difference in the processing of alternative word meanings by LHD and RHD subjects, showing that RHD patients cannot appreciate metaphoric meanings even at the single word level.

Thus, it may be concluded that the right hemisphere has a crucial role in metaphor comprehension and that literal and metaphoric meanings of polysemous adjectives are strictly separated in an individual's brain. Hence, it looks as though there are strong physiological grounds for establishing primacy of meanings. At face value, the work I have surveyed seems to weigh against the "no polysemy" view I have been putting forward. However, after I have presented here another piece of evidence on the processing of polysemes, I hope to show that the conclusion to the psychological primacy of physical meaning is not the only possible conclusion.

In a more recent study, Chobor and Schweiger (1998) came to a different conclusion regarding the understanding and processing of metaphor by RHD subjects. The authors doubted the clear-cut interpretations by Brownell at al ascribing the function of processing ambiguity to the right hemisphere, and stressed that not all of their findings had been consistent. Thus, they mention the puzzle of Winner and Gardner's (1977) study, where RHD subjects were quite able to describe metaphors verbally, which shows that they were

not insensitive to metaphor. Similarly, for that reason the findings of Brownell at al. (1990) are not consistent with the study by Winner and Gardner (1977). They also mention a number of other studies (see pp. 123-124), where RHD subjects did not display insensitivity to metaphoric aspects of word meaning, and which often provide divergent suggestions on the localization of lexical ambiguity.

Chobor and Schweiger (1998) suggested that the cognitive processes required for interpreting ambiguous words "may be more localized to the frontal lobes". One reason for such an association was the idea of Brown (1997) that metaphor and abstraction are both based on the part-whole relationship, another - that damage to the frontal lobes has long been known to impair abstract thought (Goldstein 1948). The frontal lobe has been assigned the function (Luria 1966) underlying the ability "to simultaneously hold more than one category (or concept) in one's mind" (p.121)[72] and to shift between categories, which is similar to the processes required for processing ambiguity. Thus, the authors proposed that the same mental processes are responsible for the difficulty with lexical ambiguity and for the inability to shift from one interpretation of a word to another, which is the cognitive function associated with the frontal lobe, rather than with the reduced functioning of the right hemisphere.

In their experiment, the authors tested normal and traumatically brain-damaged subjects on three types of ambiguity (homonymy, polysemy and metaphor)[73] using lexical decision and matching tasks. The experiments were designed to test whether the problems with abstraction were related to a decreased ability to deal with lexical ambiguity. The results obtained showed that on automatic retrieval (lexical decision task) there was a significant slowing in responses to metaphor among subjects with traumatic brain injury when compared with responses to homonyms and polysemes. Unlike the reaction time findings, the results of the matching task did not show any main effect for groups of subjects and the differences between categories of words (especially, polysemy and metaphor) were relatively small.

These results confirmed the initial suggestion, that subjects having problems with abstraction show reduced sensitivity to alternative meanings of words and further suggested

---

[72] The frontal lobes are also largely responsible for higher cortical sensory integration (see Kuraev et al. 2000: 209). See also chapter 3 for Cytowic's suggestion that changes in the mechanisms of sensory integration might have been a prerequisite for the development of human language.

[73] Unfortunately, the authors are not very explicit about their criteria for distinguishing polysemy from metaphor. Some examples from their appendices may be helpful (pp. 135-6). Thus, instances of metaphor included "fresh" (new) and "green" (new); instances of polysemy included only noun and verb stimuli, such as "note" (message) and "note" (music) or "march" (protest) and "march" (walk). Clearly, the examples I am interested in would have been considered as metaphors rather than polysemes.

that the difficulty brain-damaged subjects had with metaphor was that of retrieval with preserved relative intactness of semantic interpretations. They also seemed to confirm that the involvement of the frontal lobe in such tasks may be of more importance than that of the right hemisphere. Generally, the performance of the brain-damaged subjects in Chobor and Schweiger's (1998) study lead to the conclusion that these subjects had relatively intact semantic representations of the words in question even despite their retrieval deficits, which is also a possible conclusion from Winner and Gardner's (1977) study.

Those who are familiar with Chobor and Schweiger's (1998) study could object that as this study used exclusively nouns as experimental materials, it may not be appropriate to use it here as evidence since I am concerned with polysemous adjectives. A possible reply to that is that the authors did not seem to think that the difference between nouns and adjectives mattered for testing the data they wanted to test. Moreover, as they contrasted their study with those of Gardner and Winner and Brownell at al., which did use adjectives as materials, I considered it appropriate to present their evidence here as well.

Similarly, Brownell at al. (1990) raised the question of whether the adjective-noun distinction affected the reliability of their results, and concluded that "[t]hese sets of results make it unlikely that the part of speech distinction between nouns and adjectives can account for the results presented" (p. 381). They admitted, however, that in future research it may be useful to deal with that distinction more precisely. While agreeing with them on that, I will take it for granted that Chobor and Schweiger's (1998) conclusions as to the role of the right hemisphere in processing of lexical ambiguity are applicable to adjectives as well. And thus, the clear-cut distribution of primary and secondary meanings between the two hemispheres, and together with it the issue of the respective status of multiple meanings of an adjective, stop being straightforward and come into question again.

5. 3. Summary. Evidence brought together.

This is the last of the four chapters devoted to multiple meaning adjectives. In the first of them, *Evidence from Pain Research*, I considered adjectives that apply to designate sensations physiologically similar in origin, and speculated that the concepts may have a physiological basis.

However, in *Evidence from Synesthesia*, where I discussed cross-modal transfers (both perceptual and "verbal"), it became clear that it was impossible to account for linguistic

transfers (hence, the conceptual structure in which concepts corresponding to synesthetic adjectives are organized) on the basis of purely physiological or psychological hypotheses.

In this last chapter of the series, *Evidence from Double-Function Terms*, I have put forward a suggestion that the way physical-psychological metaphors are processed must be the way cross-modal metaphors are processed. I then moved on to recalling work by Solomon Asch on double-function adjectives (cross-cultural and language acquisition studies), attempting to show that an explanation from concept innateness may be better suited to his data than an explanation from mechanisms similar to that of metaphorical projection of Lakoff and Johnson's works.

Then I discussed a more contemporary psycholinguistic study of double-function terms, suggesting that a conceptual organization with fixed-flexible[74] "entries" as an alternative to a conceptual organization with a strong hierarchical favouring of physical meanings of double-function adjectives may agree with the data in a more convincing sort of way.

Finally, I have proceeded to consider evidence available from studies on processing of polysemes by brain-damaged subjects. I have presented some evidence from a recent study which put in doubt a traditional assignment of the understanding of alternative metaphorical meanings to the right hemisphere. Thus, it also puts into doubt the simple way of distinguishing between psychologically primary and secondary meanings, understood not from the point of view of frequency but from the point of view of their place in the conceptual organization.

Here is how the reasoning goes: RHD subjects are insensitive to alternative metaphorical meanings, thus alternative metaphorical meanings are psychologically secondary. But if the premise is incorrect, then there may be reasons to subject the conclusion to doubt. Moreover, since LHD subjects show greater sensitivity to connotation than to denotation, it may be that the two meanings are equally important, the preservation of one helping to retrieve the other.

Two interesting results came from the study of Winner and Gardner (1977): one is that although the RHD subjects refused to accept alternative metaphoric meanings, they had no difficulty in explaining them; the second is that no difference was found between the processing of cross-sensory and physical-psychological metaphors. Thus, my suggestion in starting this chapter has received a confirmation – mechanisms underlying the

---

[74] "Fixed-flexible entries" are here a shorthand notation for the idea that concepts corresponding to polysemous adjectives are univocal ('sharp' is the concept that gets activated whenever one comes across a sharp knife/ "sharp knife" or a sharp suspicion/ "sharp suspicion"; note that I do not say anything here on whether the meanings are univocal). More on this in the following chapters.

understanding of the two types of polysemes had better be the same for the reasons of processing simplicity.

Further, according to the hypothesis of Chobor and Schweiger (1998), the difficulty with lexical ambiguity is connected to the difficulty with abstraction, where abstraction is understood as shifting from one category to another or flexibility of thought. Such an explanation does not contradict the idea of a conceptual organization with fixed-flexible "entries" (the concept 'strong' does not correspond directly to "physically strong" and indirectly to "otherwise strong" but is rather a schematic representation of properties which gets "filled in" when combined with other concepts). Thus, the ability to shift between sensory domains and physical-psychological domains (the ability of the brain to process terms that may be applied to a variety of domains) was not lost in Winner and Gardner's (1977) RHD patients, but they had difficulty in actually switching domains (i.e., in explicitly acknowledging the applicability of a term across domains), the most frequent meaning blocking the retrieval of other meanings.

Finally, now as I have considered the empirical evidence I meant to consider, in the next chapter I will discuss theoretical issues concerning the nature of concepts and conceptual organization. The word "concepts" has been recurrent in all the works I have been mentioning so far, and in my discussions of them, everybody implicitly agrees on their existence. For all that, the question of what concepts are has not been addressed directly, sometimes creating confusion (cf. first paragraph from Brownell at al. (1990): "... work with normal populations... points to a role for the right hemisphere in processing single words and concepts" (p.375), where concepts seem to become equated with words; but are concepts linguistic units?). But as there seems to be no way of getting away from concepts, my first question in the next chapter will be: "Why concepts?"

PART III.

Chapter 6. Adjectival polysemy: what is wrong with our theories of concepts?

6. 1. Why concepts?

Georges Rey (1994) starts his article on concepts thus:

> "The notion of a concept, like the related notion of meaning, lies at the heart of some of the most difficult and unresolved issues in philosophy and psychology. The word 'concept' itself is applied to a bewildering assortment of phenomena commonly thought to be constituents of thought." (p. 185)

It would not be an exaggeration to say that concepts are tricky. Nobody knows either what they are or where precisely to look for them. Much of cognitive psychology, semantic research and the philosophy of intentional explanations is committed to concepts as those "things in the head" that are constitutive of the meanings of external linguistic items.

Here I am primarily interested in those concepts that would correspond to the numerous polysemous adjectives I have been considering in the preceding chapters. I take concepts to be those things in the head (be it in the physicalist sense of neural connections or the naturalist sense of mind-world relations) that become activated when one encounters certain strings of letters or sounds which constitute meaningful units in the language of our listener, that also become activated when one wishes to produce those strings of sounds and letters, and that are active in all kinds of inferential processing which concern the items in question. What is more, proceeding in the wake of informational philosophy, concepts are things in the head through which we are wired to the external world in the sense that the referents of our concepts in the world cause us to have them. According to Fodor (1998a), words are names for concepts[75], where concepts are mental particulars forming parts of mental

[75] Some terminological caveats. I use here Fodor's terminology which some people may disapprove of. Philosophers in the Fregean tradition would disagree with the formulation that "words are names for concepts": generally, in Fregean terms, concepts are the senses rather than the referents of words. I suppose that if we do not think of names in a standard sense as in "'John' is a name", the statement that "words are names for concepts" is not literally false: "cat" (the word) is the name for 'cat' (the concept) in the sense that whenever "cat" is encountered, 'cat' becomes activated. 'Cat' is a shorthand for whatever it is that connects us in our mental activity to "cats" and cats. I also believe that Fodor is not happy with the Fregean tradition of thinking about concepts: he wants to avoid senses so as to avoid meaning holism, i.e. for example, to avoid the possession of 'cat' being

representations. For the moment, I will also stick to the contention that concepts are meanings of words, and after I have discussed the concepts that I want to be in the head, I will return to this contention to see whether it is tenable.

The two assumptions – that concepts are mental particulars and that concepts are meanings of words – if combined, may lead to the implication that meanings cannot be shared by different people. One way to avoid *reductio ad absurdum* is to hold, à la Fodor, that taking concepts to be mental particulars does not commit one to solipsism if and only if concepts are nomological connections to the world and are as such atomic and innate. The mental state one is in when thinking about triangles does not differ across individuals, as having the concept 'triangle' means being nomologically connected to the property of "being a triangle". As long as humans are similar with respect to their perceptual processing mechanisms and their conceptual endowment, the concept 'triangle' will be shared by them. And the meaning of "triangle" will be 'triangle', that is being tuned to the property of "being a triangle" that the world may cause one to have and most often does cause one to have. As should be clear from the previous chapters I am buying both semantic atomism and innateness together with the view of nomological mind-world connectedness, and for that matter I hold that concepts are mental particulars. Likewise, I am also prepared to start with the assumption that meanings are concepts, or at least, that meanings reliably relate to concepts.

One may doubt the existence or usefulness of concepts (perhaps, after all they have little to do with the meanings of words), but the results of clinical research into the linguistic functioning of the brain suggest that concepts may be "real" and essential for one's linguistic/ semantic competence. To use the simplest example, it is clear that disturbances of certain speech areas (those affecting the lexicon) contributing to the inability to retrieve correct words are qualitatively different from, as it were, conceptual disturbances, leading to a significant loss of the ability to comprehend words and sentences (found, for instance, in echolalia). What is more, clinical research may be a rich source of information for those branches of the philosophy of mind which are not too reluctant to accept the explanatory force of "concepts". Here is one sample question: Does a patient with a colour-naming

---

dependent on the possession of 'animal' (unlike the neo-Fregean position of Peacocke 1992). I return to the question of meaning holism below.

Another non-standard usage is that of "particular". On the standard conception of particulars, if concepts are mental particulars, then there is a problem of sharing. The core of Fodor's philosophy would suggest that concepts are shared because they are innate, and thus universal. Presumably, he calls concepts "particulars" because they are "about" particular entities: 'cat' is about cats and nothing else. Such are the idiosyncrasies of Fodor's terminology, and I largely accept them here.

defect (and fully retained lower level perceptual abilities related to colour processing) have a concept 'red' if she can sort colours correctly, but under the linguistic prompt "red" puts green and red cards together? (Note that this comes nowhere near the traditional trouble with qualia – the question is not what the experiential property of seeing red is, but how the possession of concepts should be tested).

On the basis of his extensive study of naturally occurring speech errors, Garrett (1976, 1984) suggested a model of the sentence production process which included first and foremost a "message level":

> "This is not a linguistic level, strictly speaking, but rather consists of the elaboration of the basic concepts which a speaker wishes to talk about. The first truly linguistic level is the "functional level". At the functional level, lexical items are found for concepts." (from Caplan 1989: 274)

Thus, in certain cases some recurrent errors should be classified as errors of the "functional" rather than the "message" level, for instance, in cases of those agrammatic patients who have a tendency to use nominalizations instead of verbs, as in the following: (1) *The girl is flower the woman* (describing the picture of a girl presenting flowers to a teacher); (2) *The man kodaks... and the girl... kodaks the girl* (picture of a man taking a photograph of a girl – from Badecker and Caramazza 1985 as quoted in Caplan 1989: 279).

It appears that in such patients there are no impairments at the message level – the nominalizations used are at least semantically related to the verbs one would normally have used and an average listener is likely to guess at the intended meaning of the final output (hence, the patient may be said to have the concept of 'photograph(ing)' and a merged concept of ''giving' + 'flower''). On the other hand, impairments at the "functional level" are clearly evident: "to flower" (in the sense of presenting flowers to someone) and "to kodak" are not words of standard English vocabulary. Given that an average listener will not have much difficulty understanding the intended meaning and that words are names for concepts, onto what concepts will he map "flower" and "kodak" (neither of which is obviously metaphorical, thus ruling out the possibility of dismissing them as having no conceptual content)?

For philosophers and cognitive scientists, the phenomena described above are not normally considered central to the question of concepts. They tend to think of concepts as something like discriminatory capacities or modes of presentation of objects/ properties/ relations. Those presumably turn out to have realization in the brain, but this is not what makes them concepts. I also hold that functional explanations are indispensable, and that to

know where in the brain we may one day locate the concept 'love' will not be sufficient to show what makes it to be what it is. However, I am inclined to take neurophysiological data seriously: if they do not show what concepts are, they may well show what theories of concepts are not tenable, as discriminatory capacities and the like are, after all, realized in the brain.

Thus, the above questions were intended to show that there is a good case for inquiring into concepts, whatever they prove to be in the end. I shall restrict my inquiry to those concepts that probably correspond (are activated by, are evoked by, etc.) to the polysemous adjectives I have discussed in the preceding chapters. Part-of-speech distinctions are, perhaps, of some significance here: nouns pick out entities or groups of entities, verbs and adjectives are predicates, picking out relations and properties in the external world. It can be stipulated that to a certain extent they are processed differently: patients with deep dyslexia are better able to read nouns than adjectives and verbs (see Caplan 1989: 248); verbs and nouns (perhaps, adjectives too) are "orchestrated" by separate neuronal networks.[76]

This is the beginning of my story (the example was chosen arbitrarily, any other one would have done as well): presumably, there is a concept 'sharp' that is signalled whenever either sharp (objects) or "sharp" (either the written or the spoken form of the word) are attended to by us (and we are in full possession of the capacities needed to discriminate sharp objects and/or linguistic stimuli). The number of dictionary definitions for "sharp" is enormous, with the OED counting 13 entries and more sub-entries. "Sharp" spans across various domains, including reference to physical objects known by various sensory stimulations, interpersonal relations and more abstract entities: knives may be sharp (thus giving us tactile sensations of sharpness), sounds may be sharp[77], tastes may be sharp, brains may be sharp, replies may be sharp, look-outs may be sharp, attention may be sharp ("I paid him sharp attention"), etc.[78]

This is what a story about concepts (adjectives qua concepts) has to clarify: do all the various meanings of "sharp" map onto the same general concept 'sharp'? or do they all have

---

[76] See http://www.latimes.com/news/state/reports/language/lat_lang000124.htm for an article by R.L. Holtz; it is notable that describing a patient some days following a brain tumour operation, he writes: "... his speech is sometimes garbled as his brain matches the wrong word to the concept his mind is trying to express".

[77] I was told that sounds that appear "sharp" to the human ear (presumably with an abrupt or sudden onset) have a visually sharp-edged form on the oscillograph. The English language is a bit confusing in this respect, for "sharp" in music refers to a semitone higher in pitch than the intended pitch (as in "A" and "A-sharp"). So, perhaps, the English "sharp" is polysemous after all (with respect to specialist contexts).

[78] Way (1994) remarks that "a sharp dealer" is a perfectly good metaphor as opposed to the nonsensical "sharp immortality" (p.32), so it would seem that "sharp" doesn't cover just anything.

separate indexed concepts 'sharp1', 'sharp2', etc. that they relate to? or does only one of them (the prototypical "sharp") map directly onto the concept 'sharp', while the other meanings of "sharp" are metaphorical derivations and thus either mapped indirectly onto the concept 'sharp', or mapped indirectly onto some other concept following the procedure of metaphoric transformations? and if so, then how do we establish which concept it is being mapped onto?

Below I shall recount several stories about concepts to see if they satisfy the a priori requirement I have spelt out above as the black box analogy: that is, in studying language it is better to avoid redundant explanations in favour of subsuming different phenomena under more general principles. Because language (the physical internal structure of the language faculty) as we study it now is a "black box", it may also be useful to recognize "a general function F of a system in the face of F's different manifestations" (Li 1997: 176; see section 4.1). This means that the employment of the same words across the domains of sensory experience and also in the domain of interpersonal experience has to be explained by the most economical possible means. In many cases, where talk about concepts was centred round noun-concepts and verb-concepts, I shall attempt a reconstruction of the authors' views to make them applicable to the point in question.

Completing the chapters on synesthesia and double-function terms, I gave some indication of what concepts may turn out to be in my story. Let me now briefly summarize the starting point, to which I shall return after I have explicated my dissatisfaction with the existing theories of concepts. Above I doubted the possibility that many meanings of polysemous adjectives can be derived metaphorically from their "primary" meanings. The problem becomes especially clear in the case of the so-called synesthetic expressions and "amodal" adjectives. Theoretical considerations, cross-linguistic data and clinical evidence suggest that it would be sensible to treat physical-psychological polysemes alongside synesthetic expressions. This, in turn suggests that all instances of "sharp", or "loud", or "soft" are directly mappable onto the corresponding concepts 'sharp', 'loud' and 'soft'.

However, such a proposal is immediately subject to philosophical critique: how should we go establishing truth-values etc., if we cannot differentiate 'sharp1' and 'sharp2' in "sharp knife" and "sharp sound"? (Given that "A knife is sharp if and only if..." and "A sound is sharp if and only if..." will have different conditions formulated on the right-hand side). Clearly, in that case there is more to meanings than innate predispositions to detect sharpness in the real world. One may need to introduce modes of presentation corresponding

---

However, to justify this claim an empirical study of the sensible application of "sharp" would be required.

to metaphysically necessary properties in our talk about concepts or to presume that certain inferences are concept-constitutive (turning to cats again: if cats are animals, then it is a metaphysically necessary truth about them). Here, however, I want to reiterate the point that Fodor (1998a) makes: metaphysical necessity should be distinguished from semantic or conceptual necessity. In order to have the concept 'sharp' one does not need to know what makes things sharp. What one needs, in my story, is to possess 'sharp' as an innate mental schema validating the intersubjective application of "sharp" to a variety of experiences. Thus, sharp knives and sharp sounds are similar in the respect that they pick out the same kind of relation that holds between objects with keen cutting edges and the resistance objects about to be cut can offer to them and between oscillations of sound frequency and sensitivity thresholds of the human ear[79].

This will not be an obstacle to individuating "sharp (of knives and the like)" and "sharp (of sounds)". The fact that speakers of a language know that a knife's sharpness is different from a sound's sharpness comes from the sensory apparatus employed in their interactions with physical objects and sound waves. The metaphysical necessity also has its role to play: it is possible to determine expertly in physical terms what makes knives sharp and what makes sounds sharp. What is bound to be determined is that a knife's ability to cut through skin and a sound's ability to irritate the ear drum are subsumed by different laws, and thus the individuation of "sharp (of knives and the like)" and "sharp (of sounds)" is grounded in metaphysically necessary functional correspondences: physical objects exhibiting such and such properties reliably cause speakers of a language to think of them as 'sharp', and sound waves having such and such characteristics reliably cause speakers of a language to think of them as 'sharp'. However, speakers of a language need not know what reliable functional correspondences (whether necessary or contingent) are at work. In order to support the individuation of different meanings of "sharp", they only need to have a conceptual system which is reliably connected to their sensorium (knowing that sounds are not sharp in the same way as knives are only requires them knowing that perceiving and processing properties of sounds belongs to the auditory modality while perceiving and processing properties of knives does not). The point is that the possession of 'sharp' is semantically, although not metaphysically, independent of what our science and its laws may tell us about what it is for a property to be perceived as sharp. Below I discuss this claim in more detail.

---

[79] One can see that I am taking a nativistic stand with respect to the polysemes I have been discussing all along. Dretske (1969) coined the term "protoknowledge", i.e. the knowledge presupposed in perceptual claims (you can see something without seeing that it is a physical object, but for all that it has to be a physical object before you can see it). I suppose that something of this kind holds here as well: before anything may be labelled "sharp", it has to satisfy the pre-wired mental schema of sharpness, which is "amodal", i.e. not limited to a particular content of experience.

6. 2. The cognitive linguistics story.

6. 2. 1. Sweetser's Mind-as-Body Metaphor.

In chapter 1, discussing mostly Lakoff and Johnson's work, I formulated a number of objections to the validity of experientialism/ cognitive linguistics' psychological postulates, while at the same time emphasizing a number of advantages this theory has over other theories with respect to the psychological grounding of "metaphoric thought". In this section I will consider a particular set of would-be metaphors for which cognitive linguistics cannot provide a sufficiently simple and uniform explanation, and those include synesthetic expressions and double-function terms. Specifically, I will concentrate on Eve Sweetser's cognitive semantic approach to the global mind-as-body metaphor, showing that it leaves too large a gap in accounting for these two groups of expressions.

Sweetser (1990) is one of the authors in cognitive linguistics who has undertaken to study the type of metaphor/ polysemy in question profoundly and systematically. She observed that Lakoff and Johnson did not notice that individual conceptual metaphors they had detected formed part of a larger system which is the pervasive understanding of the internal self through the bodily external self, the two areas - of internal and external sensation - being analogous, and connected via metaphorical mappings. Although her 1990 book is more known for the study of Indo-European perception verbs that have dual referential function, it also contains some passages dealing with double-function adjectives which I will discuss below after reminding the reader of her general position.

Sweetser holds that semantic change proceeds the same way everywhere and that cognitive semantics alone can explain this fact straightforwardly. This explanation comes from postulating historically parallel "metaphorically structured, non-objective connections between senses" (p. 27) which have as their source humans' understanding of their experience of the world. Sense relations are thought of as non-arbitrary, and the question to be answered is how and why the vocabulary of physical perception is linked in all languages to the vocabulary of "intellect and knowledge", granted that changes generally proceed from concrete (physical and social) to abstract (emotional and mental) meanings.

So, considering double-function adjectives, Sweetser correctly remarks that a direct associative link between the physical sensation and accompanying emotions cannot be sufficient for the physical vocabulary to acquire psychological meanings. She writes:

> "But uses such as *bitter* anger and *sweet* personality seem relatively distinct from any direct physical taste-response of sweetness or bitterness. I would

> regard such uses of *bitter* and *sweet* as metaphorical: the anger is unpleasant to our emotions in a way analogous to that in which a bitter taste displeases our tastebuds." (p. 29).

Therefore, her position is that the mind-as-body metaphor may be motivated by actual correlations between external experience and internal states, but is in principle metaphorical, the understanding of internal states being modelled on the understanding of external experience.

The major reasons for suggesting a metaphorical rather than psychosomatic explanation are unidirectionality of semantic change and the large range of mappings found across the domains in question. Generally speaking, Sweetser's argument is mostly intended against semantic features analysis, which she contrasts with the metaphorical system of interconnections - highly motivated links between "parallel or *analogous* areas of physical and internal sensation" (p. 45). They include: logical necessity as the mental analogue of sociophysical force ("a strong argument"); intellection as the analogue of vision ("a clear presentation", "an opaque statement"); obedience as the analogue of hearing, etc.

6. 2. 2. Enter criticisms.

At this point, let me offer some considerations as to why I do not find the hypothesis of the mind-as-body metaphor completely satisfactory.

a) on semantic change, the idea of unidirectionality, and the validity of etymological explanations for synchronic semantics.

First of all, my doubts concern the value of etymological studies for synchronic semantics and the idea of unidirectionality. Discussing older approaches to semantic change, Sweetser writes:

> "If we took these feature-based semantic etymologies in general at their face value, the resulting Proto-Indo-European vocabulary as a whole would be an improbably abstract one." (1990: 24).

However, the same kind of objection applies to Sweetser's own approach, where it is assumed that all of semantic change proceeds from physical and concrete to psychological and abstract. If we are to believe feature-based etymologies, it appears that the proto-people had an improbably abstract vocabulary. But if we are to believe cognitive linguistics etymologies, it seems that the proto-people must have talked extensively of all sorts of

complicated physical and social states, but their talk of psychological states appeared after an evolutionary change. This seems to be highly unlikely[80].

That is, I find it problematic to believe that the physical and social worlds are more accessible to one's understanding than the worlds of emotion and reasoning (Sweetser 1990: 31), namely, that introspection and interpersonal relations become the subject of conversation and cognitive activity relatively late after the first appearance of the proto-language. Cognitive semantics holds that the "conceptual system emerges from everyday experience" (ibid.: 1), but one has to be clearer on what everyday experience comprises, as it is very likely to be related to the domain of emotions. Similarly, although introspection has not been very popular among psychologists recently, Barsalou (1999) in his article on perceptual symbol systems mentions introspection as an important source of concept formation.

And if one comes to think of folk theories, it is evident that there at least as many folk theories explaining people's character, behaviour and reasoning as the physical structure of the world (thus, physical explanations of ancient myths are often based on psychological explanations). Similarly, one can find examples when in source-languages the meanings of certain terms covered both physical and psychological domains: "Gk *katalambano*: "seize" (used metaphorically also to mean "understand") became Mod.Gk *katalambaino* "understand" (Sweetser 1990: 28). Sellars' famous (1956) story is notable in this respect. Sellars depicted an imaginary state in human pre-history when humans already possessed language, but did not have any conceptions of complex mental states (like beliefs and desires), and rather explained human behaviour in operationally defined dispositional terms. That fits my story: "strong" expresses a disposition towards a mode of interaction with objects. If pre-historical humans possessed an innate strong-schema (and no complex conceptions), then they should have been able to apply it directly to all instances of strong-behaviour.

Lass (1997)[81] made a case against unidirectionality concerning grammaticalization studies. With respect to semantic change too, the idea of unidirectionality is more likely to

---

[80] It is also possible that we have psychological modules for processing persons and relations between them or else that "each of us has concepts and processes available for inferring from the behavior of conspesifics to their states of mind... Other people are a very special part of the world; maybe one uses very special kinds of cognition when one tries to understand them." (Fodor 1998c: 154). The role of persons in our experience is not surprising even from the point of view of evolutionary continuity: it is a well-known fact that in the brain of monkeys as well as humans there are special cell populations that respond preferentially to faces and facial expressions (Gauthier and Logothetis 2000).

[81] Lass', of course, is not the most widespread view. Most historical linguists are in favour of unidirectionality in grammaticalization (see, for instance, Traugott and Heine 1991 or Traugott and König 1991), and perhaps they are right, at least on force of statistical evidence. However, I believe

turn out to be a theoretical construct, rather than pure factual evidence. Sweetser argues that the mappings from the physical to the psychological or mental (sometimes called concrete and abstract) are unidirectional. However, sometimes she herself gives examples to the contrary: "words meaning mental attention or understanding can come to mean physical hearing" (p. 35). Similarly, Williams (1976) gives the examples of English *dull, mild, soft, empty* and *keen* which were derived from non-sensory fields, and thus provides evidence that "touch and dimension, areas of relatively concrete reference, draw on words representing more abstract meanings" (p. 469). It may be argued that abstract source-words are themselves later derivatives of concrete words, but it is not clear what the value of this assumption would be, since it is unlikely that the initial vocabulary contained reference only to physical entities and events[82]. Similarly, the OED entries for various meanings of polysemous adjectives are not conclusive as to their primary and derivational nature (and moreover, there is a question of whether insignificant time differences in the first entries to distinct meanings reflect the amount of time required by that meaning to become conventionalized or whether they are mere historical accidents):

The first entry for "sharp" as "having a keen cutting edge" dates from 825
The first entry for "sharp" as "acute or penetrating in intellect or perception" dates from 888

The first entries for "bright" as "shining, emitting, reflecting, or pervaded by much light" said of luminaries or polished metals, precious stones, and other objects whose surfaces reflect light date from 1000
The first entry for "bright" as "clear or luminous to the mental perception" dates from 1000
The first entry for "bright" as [of sounds] "clear, shrill, ringing" dates from 1000

The first entry for "hot" as "adjective expressing a well-known quality or condition of material bodies, due to a high degree of molecular energy; producing one of the primary sensations" dates from 1000
The first entry for "hot" as "excited; showing intensity of feeling" dates from 971

---

that the situation is different with semantic change. Metaphor as the main mechanism in semantic change soon becomes extended to the main mechanism in conceptual change, and this is what I argue against below. It is plausible, for example, that spatials developed into temporals in English and other languages, but it is less plausible that there was a process of conceptualization (i.e., developing concepts for a domain) keeping pace with grammatical-semantic changes as many authors seem to suggest (Claudi and Heine 1986; cf. also Traugott and Heine 1991: 4 who note that the notion of abstractness used to explain the unidirectionality of semantic change is "essentially a pre-theoretical notion"). We are not only spatial beings, but also temporal beings (with probably innate spatio-temporal maps of experience – see Young 1987: 167). What presumably developed was not our ability to perceive time, but the ability to express sequentiality in discourse. In short, I argue that language does not restrict thought.

[82] Moreover, if one checks the tables Williams provides at the end of his article, it will be clear that etymological explanations are no help to cognitive semantics' approach: e.g., if the primary (etymologically primary) meaning of "bitter" is tactile, then to say that its primary meaning is about taste, with the tactile meaning being its metaphorical extension, will be contradictory.

Correspondingly, entries for those meanings that may appear more prototypical date later than entries for those meanings that would seem metaphorically derivative. Thus, the first entry for "cold" as [of soil] "slow to absorb heat" is dated 1398, while "cold" as "void of ardour, warmth or intensity of feeling" is dated 1175.

Another more general case against the applicability of etymological studies to synchronic semantics can be made from Lyons' (1968) chapter on syntagmatic lexical relations, where he mentions two ways by which lexical items change their reference/meaning:

> "the generalization of the Latin 'panarium' ("bread basket") to the French 'panier' ("basket") with the specialization of the old English 'mete' ("food") to the Modern English 'meat'." (p. 266)

My point here is that at any past time, the difference between 'meat' and 'food' must have been conceptually available (presumably, it is available even to higher animals). The naturally arising question is: how far in history must we go to find some primitive language (and a primitive conceptual system correspondingly) which would contain only a few expressions for the primitive people's thought, those primary meanings later expanding to cover all expressions known to us nowadays? Similarly, if there is no single word in English now to correspond to the Latin 'panarium', how could one ever be able to solve the translatability problem? However, as it happens, English has ways to express 'panarium' which is a complex concept 'bread basket'. Thus, etymologies provide no compelling evidence about concepts.

b) on synesthetic metaphors and direct perceptual explanations.

Let's assume for the moment that Sweetser's explanation of double-function terms is correct, but this still leaves us with a question of how we should derive the meanings of synesthetic expressions and amodal adjectives. Sweetser considers "clear" as a term proper to the visual modality that becomes a term in the domain of intellection because of the parallels between vision and knowledge. However, "clear" is applicable across the whole range of sensory modalities (one can say "clear sound" or "clear taste", etc.; moreover, "to hear clearly" seems to be as natural as "to see clearly"). Does this mean that our understanding of audition, etc. is based on our understanding of vision? If so, how would we explain the fact that not all terms from the visual modality are found in other modalities (for instance, "opaque")?

In chapter 3 I discussed Marks et al.'s (1987) research on synesthetic expressions from the point of view of psychophysics. Shen (1997) discusses poetic synesthesia and cross-modal transfers from the point of view of cognitive linguistics. But the only explanation he

offers is that mappings from more "accessible" concepts into "less accessible" ones seem more "natural" than the other way round. And accessibility is defined by two factors: the directness of contact between the perceiving sense and the perceived entity (touch and taste are more easily mapped onto sound and sight than the other way round) and the existence or lack of a special perceiving organ for a modality (touch does not have a special organ and thus is the easiest for mapping).

First of all, these explanations are not entirely plausible physiologically: touch is not an undifferentiated modality but comprises thermal sensitivity, pain detection, pressure detection, etc. (see, for instance, Kuraev et al. 2000: 181). The fact that the sense of touch is realized by cutaneous receptors which cover the whole body does not entail that there is no special organ for touch (or a separate sensory modality). Second, Shen's explanation does not provide an answer to the question why visual terms are more easily mappable to auditory stimuli, than the other way round (the fact established by Marks et al. 1987), and is generally an attempt to give a statistical rather than a conceptual analysis: it says that some modalities are a better source for other modalities, but does not say how the transfer between modalities is made possible, in particular, how and when the transfer from the so-called higher modalities to the lower modalities is achieved as a matter of principle ("loud taste"). Thus, it is not evident that some sensory domains are understood on the model of other sensory domains.

And although there are correspondences between sensory domains in terms of stimuli intensity, a direct perceptual explanation as suggested by Marks et al. (1987) does not work either (for details see sections 3.5.1-3.5.3), because it does not tell us how the terms from a particular modality acquire their meanings when applied to other modalities: 'bright music' corresponds to 'loud music' by stimulus intensity, but "bright music" does not mean 'loud music'. Moreover, the reference of "bright music" can be established with a relatively high level of precision, unlike that of "pink music", for example. The same consideration also applies to cases of simple polysemy across sensory domains: even if the perceptual relationship "hot-spicy-sharp" is physiologically valid for everyone, this is not an explanation for the conceptual correspondences found across languages: the speakers of a language know one sensation and its cause from another. Thus, starting at the level of sensory experience we arrive at a point where the fact of experiential correspondences is not yet an explanation of how such correspondences become accessible to the conceptual apparatus, and ultimately, to the language organ (if there is one).

c) on dual reference and the structure of concepts.

Now, let us return to the mind-as-body metaphor in double-function adjectives. For Sweetser, one of the instances of this metaphor is the use of taste predicates in descriptions of personal characteristics. But when she says that the use of taste predicates in talking about personal qualities operates not in the psychosomatic, but metaphorical/ analogical mode, she ignores the fact noticed several years ago by Asch that, for example, "sweetness" and "bitterness" of personalities do not simply convey a positive or negative evaluation, but have a precise range of meanings across languages: "sweetness" never means courage and "bitterness" never means overtly aggressive attitudes (see chapter 4).

Similarly with temperature metaphors: the application of "cold" and "hot" to personalities cannot be explained simply by stating that cold things are unpleasant while warm things are pleasant, because coldness in humans can mean both self-possession (Hebrew) and hostility. Nonetheless, despite the variations in the range of meanings expressed by the morphemes corresponding to 'cold' in different languages, all of these meanings are intelligible to speakers of other languages and their range is not arbitrary but obeys some generally valid principles (ibid.).

In some passages Sweetser says that "[l]inguistic categorization depends... on our metaphorical and metonymic structuring of our perceptions of the world" (p. 9), and that people tend to understand one thing as another while not considering them objectively the same (p. 8). I find it problematic to believe that there is any philosophical or psychological necessity in metaphorical restructuring of incoming information[83], which would imply rewriting it from one format onto another. If this has to be taken as a historical stance, I have offered some considerations as to why I do not find it satisfactory above. However, if it is supposed to apply to on-line linguistic processing, consider the following example: everyone knows that when you touch a cold surface, somehow the concept 'cold' is signalled; what you do afterwards is a different matter. Similarly, when you are talking to someone, and the thought "He is a cold person" occurs to you, the concept 'cold' is being activated; what inferences you draw is a different matter.

The question about metaphorical structuring can be reformulated: if it was 'cold' that got activated, then why would you translate it into 'unemotional' or whatever, and, the other way round, if it was 'unemotional' that got activated, then why would you translate it into 'cold'? A second question: if it was 'cold' and nothing else that got activated, then why

[83] Quite recently C. Johnson has proposed a theory of conflation to explain the metaphorical character of dual-reference terms in the experientialist framework. Why the theory is untenable by force of both empirical evidence and theoretical considerations is discussed in section 1.4.3.

should anyone assume that the perception of someone as unemotional had been restructured by input mechanisms to make 'cold' but not 'unemotional' available for further inferences? Generally: does the *knowledge* that "cold" is activated when touching a cold surface have any bearing on the activation of "cold" in other contexts? Or is it rather the activation of 'cold' in physical contexts itself that bears on the processing of 'cold' in other contexts?

Thus, we return to the old question of what we should call metaphorical and why. If speakers of a language, before they are able to understand "cold" with a psychological reference, have to translate it into something else, knowing that "cold" cannot objectively have a psychological meaning, then what is the cognitive value of double-function adjectives, and how should we deal with synesthetic metaphors of the type "cold light", in which presumably the same concept is being employed? Perhaps the answer is that it is more economical to use an existing word rather than coin a new one: and I have been invoking cognitive economy often enough. However, cognitive economy alone is not a sufficient explanation, although perhaps a necessary one.

To avoid any misunderstanding that might have arisen when reading the last paragraphs, I have to remind the reader that the questions I'm putting forward concern uniquely the functioning of certain polysemous adjectives with an assumedly fixed first literal or primary meaning across sensory modalities and also in the domain of interpersonal relations (such as "sharp", "clear", "loud", "bright", "sweet", "hot", "deep", etc.). I am not concerned in my thesis with the issue of conventional versus original metaphors or with the issue of models of processing – be it the route from literal to metaphorical or the choice between anomalous and interpretable expressions. I am concerned with the issue that is closer to the problem of semantic primitives (Goddard 1999, Wierzbicka 1996) and concept hierarchies (see, for instance, Williams 1992).

And thus, the main point that I want the reader to have grasped is the following: if in the continuum of "cold things", we give an explanation for the derived meaning of "cold" in the expression "cold person" such that it cannot be applied without significant changes to the explanation of the derived meaning of "cold" in the expression "cold light", then this explanation is not satisfactory from the point of view of both cognitive economy and the requirement for simplicity of a scientific explanation. From what I know of the research in cognitive linguistics on the issue and what I have mentioned above, no satisfactory explanation has been given yet within that framework.

6. 2. 3. Cognitive linguistics and implications for a theory of concepts.

If the objections presented in the previous section are taken seriously, it follows that the idea of metaphorical structuring is too general to cover all cases of polysemy and that alternative mechanisms are required. My point is not that there are no metaphors, but that not enough discriminations are being made, and that to subsume every case of polysemy under the general heading of metaphorical structuring is to obscure matters significantly. Thus, I think that the theory of the mind-as-body metaphor obscures the study of two important phenomena: the nature of concepts and the processes leading to metaphor formation. Let us consider some cases which are treated as similar but which can indeed be discriminated into separate categories.

At one point (p. 30) Sweetser mentions that the epistemic domain is structured analogously with the sociophysical domain ("a strong argument"), but it seems clear that in our primary analysis of the sociophysical domain we employ a set of concepts ('strong' as opposed to 'weak' and 'forced') which we should possess before such an analysis of the sociophysical domain is done (note that concepts and their names are not the same: other languages may not use the morphemes for "strong" and "weak" for both domains; however, the objection holds in all cases where the same morpheme is used both for the epistemic and the sociophysical domains). Therefore, when we come to analyze the epistemic domain we may well be applying the same set of concepts (and name them by the same names since we notice structural similarities) without them being mediated by our understanding of the sociophysical domain.

The same holds for concepts like 'cold': one would agree with Sweetser that there are no objective similarities between cold ice-creams, cold lights and cold people. However, there may be phenomenological similarities (or experienced properties of things in the external world). My point is that we are good coldness-detecting mechanisms, and that we possess a psychologically primitive concept 'cold' whose application (and linguistic realization) becomes triggered by the external world. The fact that we know cold ice-creams from cold people can be explained by the compositional properties of our language (and our language of thought)[84] and by its fulfilling first of all the referential function (plus syntactic and

[84] In its classical definition, compositionality is about the meaning of the whole (a phrase, a sentence, etc.) being determined by the meanings of its constituents. Both "cold" and "ice-cream" contribute their content to "cold ice-cream", and both "cold" and "person" contribute their content to "cold person". Compositionality guarantees that only those aspects of "cold" that are compatible with cold ice-creams are selected for the interpretation of "cold ice-cream" (i.e., psychological readings are prohibited as ice-creams do not have attitudes). Likewise, only those aspects of "cold" that are compatible with persons are selected for the interpretation of "cold person" (where the selection

morphological considerations). The semblance of metaphoricity comes only from the assumption that "cold" can have one objective meaning and no more[85]. The argument against metaphorical structuring is the same as everywhere else: before you map the terms from the domain of tactual experience onto the domain of psychological qualities or the domain of visual experience, you should possess a clear structure of these domains for otherwise nothing would validate the mapping. But if you do possess this structure, then how are you going to tell that you are transferring the concepts from some other domain instead of applying the same (or overlapping) set of concepts to the analysis of new domains?

In this respect, it is interesting to note that Sweetser herself calls the areas of physical and internal sensation "parallel areas", which suggests that the structural similarity between them is already there, and is uncovered in understanding rather than the structure of one being imposed on the other. Sweetser discusses the example of seeing as grasping and knowing as grasping (pp. 38-40): she states it is unclear whether the understanding of these two areas as grasping is independent or whether knowledge is grasping because seeing is grasping and knowledge is seeing. But the whole matter may be much simpler if we understand the concept of grasping as a schematic representation of action in the brain ('coming to the possession of something', where 'possession' is also an amodal concept) whose corresponding linguistic unit gets its full meaning depending on the sphere of application[86].

---

procedure is affected by contextual information: people may be psychologically cold while being physically hot at the same time - semanticists are usually embarrassed by this kind of situation, but speakers of a languages do not normally find anything to be wrong with it).

This is not, though, the strictest philosophers' sense of compositionality (for instance, Davidson's) which presupposes context-independence and context-invariance. The price that context invariance requires one to pay is assigning different subscripts to the word and making the interpreter decide which subscript to assign in the context. However, this is an explanation that many people in cognitive science cannot accept, for there is vast empirical evidence that the meanings of polysemes are related in individual words, sentence and discourse comprehension (see previous chapters). As I understand it, the reason behind having the strict notion of compositionality (besides the need to explain how speakers understand novel utterances) is to keep at a distance sceptics about meanings: if meanings can vary depending on the context, then nothing is to stop the *reductio* by which we finally arrive at a view that meanings are created exclusively by the context. The notion of psychologically primitive concepts which I introduced quite early in the thesis is in my opinion the only way to resolve the tension between a psychologically acceptable theory of polysemy (which analytic philosophy does not have) and a philosophically acceptable theory of compositionality (which people outside analytic philosophy do not have). I draw extensively on these issues in what follows.

[85] And thus the approach of experientialism and cognitive semantics to meanings turns out to be surprisingly similar to that of objectivist philosophy (or according to Lakoff and Johnson (1999), any theory which holds that concepts have to fit the world uniquely).

[86] Cf. Murphy (1996): "Although Sweetser (1990) shows convincingly that current words that have meanings related to vision also tend to have meanings related to abstract mental activity, she presents less evidence that their historical predecessors were not polysemous in the same way. Thus, it is not always clear where the historical progression is" (p. 198). Similarly, when Miller and Johnson-Laird

And thus, when we consider mental representations of these linguistic units, the most cognitively economical ways to represent polysemous terms would be to give all their meanings equal status from the representational point of view (it may differ for individual speakers by default/ prototypical specifications and frequency of access). Our dictionaries are organized quite cleverly (pace Lakoff and Johnson 1980), and something similar may be going on in our heads, the entries being specified by the names of concepts followed by the domain of application, identification procedures and, where possible, the rules of reference fixation (which may be available to experts but not to laymen). For example: grasp/physical_objects/to_grasp_an_object_you_perform_such_and_such_operations_with_your_body; grasp/mental_entities/to_grasp_an_idea_you_perform_such _and_such _ mental _ or_logical_operations, etc.[87]

These considerations concerning the representation of meanings in the brain also unravel the difficulties cognitive linguistics experiences in postulating the mind-as-body metaphor: the transition from a physical meaning to a mental meaning cannot be simple since the applicability of the same term in these two different areas is tested by different procedures. Mutatis mutandis, the expansion of the metaphor to include ideas etc. as physical objects will not be of much help: the sort of operations you perform with your ideas is different from the sort of operations you perform with physical objects (cf. Blackburn's (1984) objection to the importance of metaphor studies in philosophy).

So, in these cases when it is difficult to speak of the same psychologically primitive concept being at work, some other mechanism should be able to guarantee the possibility of inter-domain transfers. And this mechanism is most likely to be the mechanism of analogical reasoning, extensively studied by psychologists (see, for instance, Sternberg 1977 and 1982, Gick & Holyoak 1980, Holyoak 1982 and 1985, Holyoak and Thagard 1989, Michalski 1989, Vosniadou 1989). In her example of the sociophysical and epistemic domains, Sweetser speaks of logical necessity as being the mental analogue of permission or ability in the real world. My point was that for some predicates we should rather speak of the same primitive concept spanning a variety of domains. In other cases, where we find no such concepts, we can speak of analogical reasoning being employed for the comparison of the two domains (but analogical reasoning is primarily a method of theory formation, not

---

(1978) discuss the verb "see", they suggest that the principle applied to its different meanings may be that of implication (pp. 584-5), rather than figurative transformation.

[87] It is interesting to note that the tendency towards the kind of "unification" I am pursuing in my thesis is very much in the air nowadays. In a quite convincing article Stoffregen and Bardy (forthcoming) argue against the assumption of separate senses in favour of global specification. I am arguing against the often blurred and shaky literal-metaphorical distinction in favour of a more psychologically consistent view of concepts.

concept formation). And I think that for this reason one should be careful in distinguishing analogical understanding from metaphorical understanding: in one case we transfer the explanatory principles for the relations within a domain, in the other - these relations themselves (this is explicitly claimed in Dejong (1989); many psychologists involved in analogical reasoning research also note this point and often in developmental perspective – see, for instance, the works of Gentner (Gentner 1983, 1988, 1989, Gentner and Stuart 1983, Gentner and Toupin 1986, Gentner et al. 1988, Gentner and Clement 1989, Medin et al. 1993, Bowdle and Gentner 1997, Gentner and Wolff 1997); see also section 1.6.2. for some examples in which the transfer of relations is not much help in scientific reasoning).

To sum up[88]. Further extending arguments from Chapter 1 against experientialism as an acceptable theory of concepts, in this chapter I have discussed Sweetser's version of cognitive linguistics – the mind-as-body-metaphor, showing that for a variety of reasons, including reasons of cognitive economy, cross-domain transfer facilitation, and finally the explanation of the black box analogy, this version is unacceptable as a theory of concepts. However, despite the severe limitations that the cognitive semantics approach may be suffering in connection to its psychological applicability, it still possesses a vast resource of unexplored potentialities worth taking up.

One of those potentialities is the idea of concepts as mental schematic representations of relations (although not exclusive to cognitive linguistics, as we shall see below), which are part of our biological endowment just as some schematic representations (e.g., patterns for visual processing, cognitive maps, etc.) are a part of the biological endowment of other animals, and whose corresponding linguistic units acquire their meanings depending on the sphere of their application in the course of human linguistic development. But to accept this would mean to give up some empiricist assumptions that are implicit in the experientialist philosophy persistent not only in Lakoff and Johnson's but in Sweetser's theory as well.

---

[88] Much of the argument I have been exploring here may be found in Murphy (1996). Murphy emphasized "structural similarity" between domains one of which becomes mapped onto the other, and where "metaphors arise out of the similarity of pre-existing conceptual structures" (p. 179). Similarly, he argued that "Inflation is rising" is not a metaphor, because "rise" has both physical and abstract meanings, and underlying them is the abstraction of undergoing a change such that the value of an entity on a dimension increases (given that "increase" is univocal; p. 190). In spite of this, I decided to attempt in this section another exposition of cognitive linguistics views, since of primary interest to me are synesthetic and double-function polysemies, where it is unclear that the structural similarity between domains view is of much help – see above for the "bright music" example.

6. 3. The lexical semantics story.

6. 3. 1. Lexical semantics and meaning holism.

Lexical semanticists (or at least, many of them) form another category of researchers who are vitally interested in concepts and conceptual structures, presuming those to be at the heart of the issue of meaning for linguistic items. Therefore, in this section I will be drawing mostly on the fundamental work of Miller and Johnson-Laird (1978), whose approach of semantic decomposition is at the basis of later work in lexical semantics.

In their *Language and Perception*, Miller and Johnson-Laird proposed to give a psychological theory of semantics or "psycholexicology", that would relate reliably perceptual and lexical structures as mediated by the conceptual structure:

> "It soon became obvious that the correlations we had noted between perceptual and linguistic structures were mediated by an enormously complex conceptual structure. Space, time, and causation are concepts. Percepts and words are merely avenues into and out of this conceptual structure. Any theory of the relation between perception and language must necessarily be a theory of conceptual thought." (p. vii)

Assuming that sensation, perception and cognition cannot really be separated, the authors started with the contention that since words like "red", "loud", "sour", etc. are found across languages, there must be some real physiological and psychological processes correlated with their use. Thus, even though some theorists did not regard such entities as what there truly is to perception, the latter were taken to be "the basic atoms of the mind". This way the perceptual theory served as a grounding for the conceptual system: perceptual properties and relations are represented in the form of predicates. To construct a theory of concepts was not (as it never is in lexical semantics) Miller and Johnson-Laird's first task: their objective was to provide a psychological theory (hence, only by extension a theory of concepts) which could explain the fact that certain judgements become expressed in human languages (see p. 36). However, there are some problems with the resulting picture, and I presently turn to them.

According to Miller and Johnson-Laird's story, the conceptual system (or the lexicon with its internal representations) must cater for "both extensional and intensional rules for assigning meanings to expressions" (see p. 290). The reason for introducing the extensional part is quite clear: the origin of concepts is, as it were, in the perceptual and functional identification of instances ("attentional-judgmental abstractions from perception"). The

intensional or connotative part is a little bit more complicated. First of all, it comprises two types of mental entities: conceptual cores and lexical concepts.

Conceptual cores should not properly be called "concepts" as they are indeed prototheories or schemata acting as primitives for other schemata. To quote from Miller and Johnson-Laird again:

> "science may introduce new words; "mammal" was inserted between "dog" and "animal" when animals were systematically catalogued. Such changes do not alter the denotation of words already familiar, but they do reflect profound changes in meaning – in the conceptual core underlying those words." (p. 291)

Similarly, 'person' (or 'space', or 'causation') is a concept that does not have any lexical field associated with it, and in some sense is a "core concept" which serves as an underlying basis for a lexical field and which organizes representations of all the general knowledge and belief one has about those creatures that may be denoted by words in that field.

Thus, another constituent in the intensional part of Miller and Johnson-Laird's story are lexical concepts. Lexical concepts are those internal representations that correspond to words, that are organized into lexical fields around a conceptual core, and that acquire their particular location in the lexicon only relative to other lexical concepts in their field (the field of kin terms, for instance; cf. the notion of sense-relations). Apart from that, the existence of conceptual cores is supposed to be a kind of guarantee that lexical concepts can be extended and figurative uses interpreted. Generally speaking, meanings for Miller and Johnson-Laird cannot be given just by denotations; what is also needed are "intensional properties of linguistic expressions" and "the intensional relations between them", thus giving access to encyclopaedic information in long-term memory (see p. 706).

I will pause here for a moment to show what is wrong with such a theory of conceptual structure, and then proceed to analyze Miller and Johnson-Laird's contentions about concept development and adjectival polysemy which are more directly relevant to the purposes of the whole discussion. Let's begin with the authors' own example – the lexical concept 'mammal'. According to Miller and Johnson-Laird, with the introduction of "mammal" into scientific discourse, there was no change in the denotation of "dog" and "animal", but the meanings were changed, where meanings are understood as dependent upon the conceptual field where they belong and their relations to other lexical concepts in that field.

This is one of the many versions of inferential role semantics (IRS) or semantic holism (or, perhaps, strong molecularism), where the meaning of a term is considered only relative to some other terms in the language. So, what did "mammal" do to "dog" and "animal"? Presumably, dogs remained recognizable as dogs, and animals remained much the same: if

you called something an animal before the change, you kept doing it without any reservations after the change. Thus, it must have been the way you represent dogs or think about dogs. A lot hinges on the existence of analyticity: if you used to think of dogs (hence, animals), now you think of dogs (hence, mammals, hence, animals). If there are no analytic sentences, i.e. nothing that you have to believe in order to believe that there are dogs, apart from believing that there are dogs, then the introduction of "mammal" brought no changes to the meaning of "dog". (Analyticity is not something that I will discuss any further; see Quine's (1953) "Two Dogmas of Empiricism").

By Miller and Johnson-Laird's story, the introduction of "mammal" affected not only "dog", but "cat", "cow", etc. – every single term in the field of "animals". However, as it happens, we tend to think of animate beings in a unificatory way. "Animate" subsumes both "animals" and "birds", so the two subfields are really a part of the bigger field of "animate beings". Thus, if it is granted that thinking about dogs requires being able to think about cats or cows, it might well require being able to think about chickens and alligators. Hence, the introduction of "mammal" must have affected the way we think of chickens (since we tend to unify animate beings and separate them from inanimate beings). Whenever you now think of a chicken, you think of its being "non-mammal". The story may go *ad infinitum*.

The above was a brief illustration of the kind of problems inferential role theories encounter trying to provide for meanings. The standard criticisms of them (see Fodor and Lepore 1992) apply here as well: what holistic theories finally lead to is that you cannot believe anything (or know the meaning of any word), unless you believe all other propositions there are to believe (unless you know the meanings of all other words there are in the language). Evidently, this makes it hard to explain how communication or language acquisition are possible and undermines the possibility of scientific knowledge (naturally, on condition that one believes in the accumulation of human knowledge as I do, rather than in the change of paradigms which would make all of the above only provisional). From Miller and Johnson-Laird's example it would follow that before the introduction of "mammal", when we spoke of dogs we spoke of something different from what we speak now when we have the lexical concept of "mammal", or even, that we could not have been speaking of dogs at all (since we did not speak of them as dogs, hence, mammals, hence, animals). It is much more likely that dogs themselves have not changed, and that even "dog" has the same extension as before[89].

[89] There are many people who believe that it is indeed the case that "dog" has changed its meaning several times and is likely to change it again, thus that our speaking of dogs is different in all those time slices. However, as natural kinds are not my concern here, I cannot go into discussing the issue at great length. From the point of view tacitly accepted in my thesis, if one's "tuning" to dogs was the

6. 3. 2. A note on meaning holism and conceptual development.

Another standard criticism of inferential role theories of meaning is that they inevitably pose the problem of learning (see Dummett 1973: 599-600): if knowing what a term means involves knowing what quite a lot of other terms in the language mean, then acquiring the meaning of a term poses an unresolvable problem. Unless, of course, there are two categories of terms – those learnable independently, and those whose meaning hinges on the meaning of other terms (somewhat like Locke's primary and secondary qualities).

Let us now consider how Miller and Johnson-Laird (1978) deal with the problem of learnability and whether they manage to avoid the trap of saying that both the following statements are true at the same time: that children possess essentially the same concepts as adults and that initially children have concepts differing from those of adults and only eventually come to have the same concepts as adults have, i.e., that "a child has a concept [causality] that is *both* identical and different from the adult's" (Rey 1994). The main objective of this section is therefore to show that meaning holism is not only unnecessary, but also troublesome in explaining concept acquisition.

Miller and Johnson-Laird's perception is that people speaking the same language necessarily share core concepts implicit in that language or some basic theory that those core concepts are about. However, they stipulate that being "personal things", concepts allow for individual differences, a fact which explains occasional failures of communication. But, as they say,

> "conceptual differences are usually peripheral ones, not core. For example, using 'doggie' to label a cat indicates a difference in conceptual refinement, not in the core concept; using 'doggie' to ask for a drink of water violates the core concept." (p. 292).

One way to understand this (which would also be in certain agreement with the idea I have been expressing in my thesis) is to suppose that having shared perceptual apparatus leads to the possession of such a core concept as 'animal' or 'middle-sized animate object exhibiting certain behavioural characteristics' (or else 'living being'). In a sense, the possession of this type of concepts is a "pre-wired" ability. The stranger it is then to think that "doggie" could be used to ask for a drink of water. Thus, violations of core concepts must be unlikely to happen in real life situations. All that a child has to do is to master the perceptual-conceptual refinement of 'middle-sized animate' into 'dog', 'cat', etc. But that

---

same before the introduction of "mammal" as it has been since the introduction of "mammal", then one's tuning to "dogs" has not encountered any changes significant from the point of view of concept possession.

would show that, contrary to some of Miller and Johnson-Laird's assumptions, 'dog' and 'cat' are independent, not interdependent, concepts[90]. Young children may not be able to tell reliably dogs from cats. Their rectifying 'middle-sized animate' into 'dog' requires a higher precision perceptual apparatus, which is a step in development. Although the ability to tell dogs from cats presumably appears at the same time as the ability to tell cats from dogs, the ability to discern dogs does not depend on the ability to discern cats (if there are no cats in a child's world, the ability to tell dogs from other animals will appear all the same). Hence, at the level of "core concepts" conceptual atomism is wholly viable and basic concepts are the same for children and adults.

What then are peripheral differences? For Miller and Johnson-Laird, they are individual differences in concept possession caused by difference in age and education, i.e., the theory of meaning turns out to be a theory of knowledge organization at the same time: the more you know, the "better" meanings you have. This, however, has long been a problematic view in semantics: it presupposes that concepts are epistemic capacities, and deciding what kind of knowledge counts as concept-constitutive hinges heavily on the existence of the analytic-synthetic distinction. We may assume that communication is a matter of *approximate* matching between the concepts held by different speakers, but this will be no help in trying to reduce concepts to epistemic capacities. A child who uses "doggy" to name a cat communicates (even if for her "dog" is 'anything that is like that creature over there', and we are professional breeders). A child who uses "doggy" to ask for a drink of water does not. Peripheral differences, therefore, are not concept-constitutive. And we are back to where we started: concepts understood as core concepts cannot be different for children and adults.

Where then, according to Miller and Johnson-Laird, does the difference between children's and adults' concepts lie? Apparently, in the last stage of conceptual development – the organization of lexical items into fields and their falling into particular locations in accordance with the core concept. But we are even further back now. For the difference turns out to be not whether one would reliably classify perceptual objects consulting the

---

[90] I do not think that any of "not cat", "not giraffe", "not horse", etc. or else their conjunction is part of the meaning of "dog" for the reason mentioned in the previous section: we may go *ad infinitum* trying to establish which of "not ..." or "not... & not ... &..." is constitutive of the meaning of "dog". And it is doubtful that we will ever arrive at a conclusive definition, unless we are prepared to accept that the conjunction of terms for all known middle-sized animals is constitutive of 'dog'. The fact that 'being a dog' and 'not being a cat' go together in our reasoning has to do not with the concept 'dog' being complex, but with the statistical prevalence of "cat and dog" discourse situations. (Consider: when we think of 'dog' being complex, we are more likely to suggest 'not cat' as constitutive of it rather than 'not cow'. However, cows are as good instances of middle-sized animate objects as cats are).

core concept one has, but in how much one knows of the relation in which any two terms in the field stand to each other (associated knowledge and beliefs).

To take the view to its extreme: if you know quite a lot about the beliefs that dogs have about cats and cats have about dogs, and whether either have any beliefs at all, which I don't, then your concepts 'dog' and 'cat' are peripherally different from my concepts 'dog' and 'cat', and the only problem we have to solve is to show whether being different peripherally counts as possessing different concepts, which I think it does not. If we take the view even further than that, then we shall come to a position in which nobody can truly have a concept (or else have a true concept) about either dogs or cats unless one knows everything there is to know about them (the God's-eye view). However, it is unclear that this would be relevant to our current talk about concepts, since the only thing they have to do is to connect perceptions with words – this is achieved in terms of core concepts, which are the same for everyone and which are the warranty for successful communication between parent and child on the inconclusive basis of the child's linguistic output (Miller and Johnson-Laird 1978: 293).

6. 3. 3. Lexical semantics and adjectival polysemy.

The most important reason for discussing here the lexical semantics story about concepts is the issue of adjectival modification and its implications for the theory of conceptual structure. Thus, Miller and Johnson-Laird make a passing remark:

> "Perhaps it is a mistake to think that the concept expressed by a word like "sharp", which can describe touch, taste, sound, intelligence, terrain, strictness, eagerness and objects, is legitimately applicable to touch and must be generalized for other applications; SHARP may be a concept of more than just a sensory quality... A sharp mind may be as good an instance of SHARP as a sharp pain; a warm person may be as good an instance of WARM as a warm tactual sensation." (p. 360)

By the end of the book, having decomposed a number of semantic fields into their primitives – core concepts such as CAUSE, TRAVEL, POSSESSION, etc. plus the rules of modification – they suggest further reductions in the conceptual structure (concepts being associated with lexemes, not words):

> "Can all these uses be assimilated into a single concept [MOVE]? We have made no attempt to provide a different formal schema for each slightly different sense but have assumed that a single schema, supplemented by some unspecified procedure for modulation by context is involved in each case... We prefer the view that there is a lexical concept common to almost all these senses

> and that the meaning of a word can be extended in only some finite number of ways without baffling native speakers." (pp. 676-677)

The above quotes illustrate what is one of the major attractions of such approaches: the reduction of all lexical and conceptual variety to a number of primitives spanning and unifying all conceptual fields (cf. ch.1 on reductionist tendencies in experientialism; cf. also Pustejovsky's (1995) claim to avoid unnecessary proliferation of meanings). Considering in the previous chapters instances of polysemous adjectives and their possible psychological grounding, I also found the idea of psychologically primitive concepts attractive. However, if 'sharp' is indeed a "unificationary" concept (and correspondingly such concepts as 'move', etc.), it poses a problem for the rest of Miller and Johnson-Laird's theory of concepts, as well as any other inferential role theory of meanings/ concepts.

In the beginning of this section I stated that according to Miller and Johnson-Laird one of the major requirements for a semantic theory is to provide a psychological theory of usage. One step in that direction was a theory of conceptual structure, comprising extensions and intensions. The intensional part of the semantic theory was considered no less important than the extensional part and included core concepts and lexical concepts. How, in compliance with these distinctions, should one treat concepts like 'sharp'?

If 'sharp' is a dispositional concept, then in its extension there are all instances of sharpness to which, under normal circumstances, we are prepared to respond as such in our experience of this world, and perhaps all other possible worlds also (although we may happen to be wrong about what creates this disposition, and its extension may turn out to be different from what we think it is). The extensions of "sharp object" and "sharp taste" will differ inasmuch as the extensions of "object" and "taste" differ. Thus, all there is to 'sharp' is given in its extension. Moreover, 'sharp' appears to be one of those concepts which do not have lexical fields associated with them. (There are, of course, lexical items that are associated with 'sharp', such as 'blunt' for instance, but what lexical field do they belong to? It is not the same as 'man' belonging to the field of 'living things'[91].) Thus, 'sharp' may be considered a core concept, but this will add nothing at all to knowing what is in its extension.

However, if 'sharp' is not a core concept, then by Miller and Johnson-Laird's theory it can only be a lexical concept, and thus dependent on a core concept and other concepts in the same (mental) lexical field. And here we run into a vicious circle: if 'sharp' is a lexical concept, one has to specify a field for it, and if the field is specified (for instance, tactile

[91] Where 'living being' is the core concept, and 'man' is a lexical concept. While 'living being' is relatively stable, 'man' is likely to be affected by conceptual changes in the field of 'living beings'.

sensations), then one has to explain the derivations of all other 'sharp'-instances (not only across domains but also within the domain of sensory qualities) – the very reason that made Miller and Johnson-Laird suggest that 'sharp' may be the same concept applicable across a number of domains. On the other hand, if it is the same concept, then the semantic theory can do without intensions for a large number of meanings[92].

Less evidently, the same vicious circle arises for the concept 'move'. If all instances of "move" can be assimilated under the same concept, then all we need to know is the extension of 'move' (the type of relations that can be properly described as "moving"). And thus, in both "He moved the table to the window" and "His words moved me deeply" 'move' denotes the same type of relationship. If 'move' is nothing more than a lexical concept common to the two uses, then one has to have a core concept and other lexical concepts in its lexical field which would delimit the application of 'move'. However, lexical fields for the two senses of "move" will not necessarily coincide. If the meaning of "move" depends on whether there is in the language the means to express the action of pushing, then it is dubious that the two senses can be unified by the same concept ("He pushed the table to the window" vs *"His words pushed me deeply"). Thus, if theories of inferential role semantics wish to avoid the proliferation of meanings, the only way they can do it is to give up the idea of the meaning of one term depending on the meaning of the other terms in the language, the assumption which is central to them.

The problems of polysemy and meaning proliferation in lexical semantic approaches mentioned above have been discussed in detail in some of Fodor's works. In the previous sections I have also been arguing for concept/ meaning atomism with respect to polysemous adjectives. I have also suggested that the various meanings of a polyseme can be subsumed by the same psychologically primitive concept. Strikingly, although largely consistent in other respects, Fodor's views on polysemy, as displayed by his critique of Jackendoff and Pustejovsky show certain puzzling inconsistencies. Therefore, I will not discuss Jackendoff's and Pustejovsky's lines of reasoning independently but turn instead to Fodor's critique of Jackendoff and Fodor and Lepore's critique of Pustejovsky to see what other problems, apart from those mentioned above, lexical semantics may have in studying polysemy and whether informational semantics is an answer to the "sharp"-problem. Finally, summarizing briefly the present section, I should remark that the positive (for my current purposes) outcome of Miller and Johnson-Laird's theory is the idea of the same concept being employed across a variety of domains, thereby rejecting the metaphoricity of

[92] As it happens, there may be other reasons for bringing intensions into the picture – for instance, in order to explain how "This could have been sharp" may be true when said of something blunt.

a number of expressions (and corresponding representations in the mental lexicon). The negative outcome and the hindrance to that idea is the inferential role theory of conceptual cores and lexical concepts for the reason that the dependencies within the mental lexicon are unclear, and that, on the condition that those differ across individuals depending on age and educational background, the concepts possessed by different individuals become necessarily too different to be of any use for a psychologically grounded theory of concepts.

6. 4. The informational semantics story.

6. 4. 1. Fodor's critique of Jackendoff.

The variety of notions I have been exploring above – concept innateness, conceptual atomism and concept causation by the world we are in – have largely been borrowed from the works of Fodor (1980, 1987, 1998a, 1998b). His informational semantics (Fodor 1998a) claims that meaning is information in the sense that the content of a concept (what it refers to in the world) plus modes of presentation of that concept to the mind are all one needs for concept possession. No epistemic connections with other concepts are required, and to preserve this independence of symbol-world relations, most concepts have to turn out to be primitive or unlearned.

Contrary to this trend, lexical semanticists are prone to establish a number of primitives in a conceptual domain which serve as a basis for deriving all other meanings of the terms in that domain (hence, concepts - since terms in a language correspond to concepts in a mental lexicon). The famous story of killing as causing to die illustrates the point: some verbs are conceptually primitive, while others are derived from them by a number of operations, application of the concept of causation being one of them. Fodor's objection (1970) to "killing" meaning 'causing to die' is no less well-known: if you poison someone on Friday and he dies the next day, then you do not kill him and cause him to die on the same day, and thus the two expressions are not semantically equivalent. Jackendoff (1992) developed this traditional line of thought for lexical semanticists and came under Fodor's (1998) attack.

Finally and most importantly, some ideas of lexical semantics, such as the idea of a concept acquiring its "full" meaning depending on the field of its application, have been explicit in my treatment of polysemes ('bright' as one and the same concept spanning a variety of domains; 'grasp' as one and the same primitive concept whose testing procedures depend on its sphere of application, etc.). However, Fodor's disquotational lexicon (i.e., the

lexicon that contains only extensions and logical forms) does not allow polysemy. Thus, to find a way of preserving both the idea of concept identity in so-called polysemous terms and Fodor's robust notion of compositionality is the task for a good deal of this section. If no such way is found, one of the assumptions will have to go.

In a chapter against definitions as constitutive of concepts, Fodor discusses Jackendoff's (1992) proposal for formalizing "keep" ("formalism for encoding concepts" – see p. 49), whose meaning (the concept it expresses) is supposed to be an unchangeable 'cause a state that endures over time'. According to Jackendoff, in all its usages (from the expression of possession in "Susan kept the money" to ascription of properties in "Sam kept the crowd happy") "keep" has the same functional structure and denotes causation. To put it in familiar terms, the underlying concept for all "keep" instances is 'cause a state', and the meanings of "keep" in all particular instances are derived somehow from this concept plus the semantic fields in which it operates.

As Fodor rightly remarks, to make this explanation work one has to assume that 'cause' is univocal across all semantic fields, and if 'cause' can be univocal, then why can 'keep' not? (p. 52) The solution Fodor offers[93] is that there is no polysemy (read: no definitions). Thus, "keep" always has the same meaning, because it expresses the same relation in all contexts, the difference being that in one case the relation is between NP and the money, in the other - between NP and the crowd's being happy:

> "To be sure, if you are committed to 'keep' being definable, and to its having the same definition in each semantic field, then you will have to face the task of saying, in words other than 'keep', what relation it is that keeping the money and keeping the crowd happy both instance. But, I would have thought, saying what relation they both instance is precisely what the word 'keep' is for; why on earth do you suppose that you *can* say it 'in other words'?" (p. 55)

That is, the difference in the meaning of "keep" that one intuitively feels is there depends on the content of what it is a relation to. Fodor suggests that "roughly" "Susan kept the money" is a variant of "Susan kept having the money", while "Sam kept the crowd happy" is a variant of "Sam kept the crowd being happy" (p. 54), and thus the difference between the two is that of the surface form. I find it difficult to agree with this last bit, because it exhibits as much circularity as Fodor accuses Jackendoff of - one will have to say that in one case "keep" means 'keep having', in the other - 'keep being', the difference between the two to be sought in the difference between the money and the crowd's momentary happiness

[93] Rather, one of the solutions. The alternative view is that "keep" is merely ambiguous.

- so one does not dispense with semantic fields after all[94]. So, contrary to Fodor, there is a "formalism for encoding concepts" (whatever it turns out to be in the end) in the content of 'keep': it is exactly what makes the relation between NP and the money and between NP and the crowd's being happy the same relation.

But essentially, I think that Fodor is right - the above two instances of "keep" do indeed have in common the fact that they are both instances of *keeping*, i.e., some particular relation in which one stands to bits of the world, and which is different from holding or kicking, or anything else. And this is why I think that his informational theory of meaning is right too - if we perceive the relation as the same in both cases, then why shouldn't we say so? However, the trouble is that Fodor does not go into such niceties as showing where this sameness comes from, i.e. why anyone should perceive the instances of keeping as the same. His position (when he does not think that "keep" is ambiguous) is that "keep" is not polysemous, but always has the same meaning. In short, 'keep' is an atom, and no definitions are there to explain the intuitive difference between keeping money and keeping the crowd happy.

One more point deserves attention now: the issue of translatability. Fodor asserts (p. 55) that one has to distinguish questions about meanings and questions about possession conditions. "Spoiled" and "addled" are synonymous, they will be both correctly translated by "spoilissimoed", but their possession conditions differ (you have to know that "addled" is said only of eggs). On condition that all languages have the same expressions (hence, same concepts) for the same sorts of thoughts, one would be right in saying that 'keep' is primitive. But if they do not (as happens in Russian, for example, where 'keep' and 'hold' are expressed by the same word in some contexts – "derzhat'", while in contexts of possession a completely different verb is required – "hranit'"), but, intuitively, the same pattern of expressing the relation of *keeping* is being preserved across languages, then one has to give an explanation for this variation, which is variation upon essentially the same relation. For that reason I preferred to start with examples of a different kind – cross-modal transfers and double-function terms – which exhibit uniformity cross-linguistically.

The above should not be read, however, as if I were proposing to equate concepts with words. I am just trying to break through the tradition of monolingualism. I would speculate that with respect to 'keep', 'hold' and its Russian equivalent 'derzhat'', one can speak of 'keep' and 'hold' being synonymous as Fodor wants it (note the single quotes – we are

[94] And the notion of the "surface form" is no help here either. We can have both "Susan kept the money (being) hidden" and "John kept the crowd" (= kept having the crowd's attention). If the difference between 'keep having' and 'keep being' is not in the complement clauses, then it has to be the difference in semantic representations of 'keep', and thus we are back where we started.

dealing with concepts, not expressions of the English language). And thus, we get some primitive concept expressing the relation of 'keep-holding' for all human beings, but the ways it gets realized in different languages (possession conditions) may differ. Cf. the story about "hot" and "sharp" taste sensations.

Interestingly, the misreading of concepts as words is one persisting misinterpretation of Fodor. Thus, this is what Schiffer (1998) does. Arguing for the Informational Role Semantics account of meanings and disapproving of Fodor's publicity constraint on concepts, he writes:

> "Moreover, Fodor clearly intends the publicity constraint to hold even for thinkers who think in different language of thought. If I think in neural English and you think in neural French, it should still be possible for us to share innumerable concepts." (p. 15).

He doesn't seem to notice that the same objection is even more valid for IRS - a French definition is not the same as an English definition (i.e., what words used in a definition mean) - there are too many conceptual relations involved to ensure the sameness of reference. Hence, the old gavagai problem.

Schiffer mistakenly thinks that if there are "neural words", they should stand in one to one relation to our spoken and written words, and thus concludes to the validity of IRS as opposed to conceptual atomism. However, even with respect to cross-modal and double-function terms, as I have argued above, atomism is perfectly plausible. The fact that all languages I happen to know about have ways of expressing the concepts 'hot' and 'cold' does not depend on our conceptual or lexical organization, but on our being members of a species for whom the ability to feel hot and the ability to feel cold matter in some significant respects (e.g., they've got to do with pain thresholds, which have little dependence on our wishes to feel or not to feel certain stimuli).

Returning now to Jackendoff, I would claim that his theory of concepts cannot be a theory of concepts corresponding to polysemous terms, even despite the attractiveness of the idea of underlying conceptual formalisms (as in all meanings of "keep" being subsumed by the primitive 'cause'). For one simple reason. With respect to polysemous adjectives, it would require that the concept 'bright' be univocal not because it is an atom, but because the meaning of "bright" in all its instances is given by the concept of 'causing sensation X' (where X is to be specified) plus the semantic field feature (hence, definitions; hence, complex concepts). Contrary to that, in chapter 3 I argued against biological reductionism reflected in reducing concept identity to the similarity of sensations (bright lights and bright music cannot produce the same sensation). However, if 'bright' is an atom (hence, an innate

concept), the similarity or dissimilarity of sensations does not affect its possession conditions: if you can interpret "bright" in one semantic field, you have all the potential you need to interpret it in a different field.

One final remark. Jackendoff (1998) in many ways repeats the views he espoused in 1992, although taking an even more pessimistic stand on concepts. He still holds that concepts are complex (e.g., adopting Pustejovsky's notion of qualia structure) and that the lexical-conceptual structure should incorporate things like "world" knowledge (p. 31), thereby resolving the problem of adjectival polysemy and adjective-noun modification (p. 66). Considerations from adjectival polysemy, among other things, led Jackendoff to contend that the idea of language as thought is illusory. Thus, "good" has far too many meanings depending on the context, and its interpretation should be somehow constrained by "world" knowledge; therefore, there is not a simple concept 'good' corresponding to all "good" instances. Similarly with "unconscious": it has such a range of meanings in common usage that "we don't know whether the word expresses one bendable concept or a family of more rigid, related ones" (p. 206).

The same consideration works the other way round: we possess concepts for which we have no words. Jackendoff's example here is the concept of belief: if it is taken to be propositional, then we cannot apply "belief" to animal reasoning and have to invent other lexical entities when talking about animals' mental abilities. Thus, "there happens to be a gap in our vocabulary" (p. 207). The general conclusion is that we are being deluded by the phonetic forms of the words we use to express our thoughts into believing that they are thoughts; therefore, thought and concepts are, as it were, sub-semantic and not fully expressible linguistically:

> "Because a word is a constant percept in our experience, we treat the thought it expresses as a constant thought – even though in fact we bend and stretch the concepts expressed by words every which way, especially in the process of combining them into sentences by coercion and cocomposition" (p. 206).

Jackendoff's hesitation in accepting the conclusion that the same concept may be at work when we predicate "belief" of humans and animals or when we predicate "good" of weather and children suggests that he may be unhappy with his own treatment of polysemy. If the concepts 'good' and 'belief' are not bendable, then there should be several concepts corresponding to instances of "good" and "belief", but this would take us back to the idea of the enumerational lexicon, something that lexical semanticists wish to avoid when dealing with polysemy. On the other hand, Jackendoff does not think that all instances of "good" and "belief" uniquely correspond to the concepts 'good' and 'belief' (he finds it

embarrassing that good weather and good children may have something in common – I return to the "good" examples in the next section).

A way out of this is, according to him, to think of concepts as being bendable depending on the context. But this is almost the same as to think of concepts as being vague (consider: if "good" has meaning only in the context, it follows that there are no cognitive restrictions on the application of concept 'good', i.e. that the concept 'good' is vague and that anything goes in linguistic processing). However, vague concepts do not compose: try thinking vaguely of children being vaguely good. Since Jackendoff admits that thinking is computation (his acceptance of modularity confirms that), he cannot let concepts be infinitely stretchable. But as he denies that words correspond to concepts one to one, he can no longer believe in "concepts encoding formalisms" (or the reduction to primitives) he put forward in 1992. In the next section I further discuss some problems that lexical semantics encounters in its consideration of polysemy, and in chapter 7 I attempt to present a different way out of Jackendoff's puzzle.

6. 4. 2. Generative lexicon and the problem of polysemy.

The expression "to avoid the unnecessary proliferation of meanings" has become by now a slogan for those who work in the tradition of Pustejovsky's (1995) *Generative Lexicon*. The interesting point for us here is Pustejovsky's treatment of adjectival polysemy, where he claims that the interpretation of a number of adjectives depends on the so-called qualia structures forming part of representations for nouns. This way we can say that an adjective has only one meaning (underspecified outside the context) and do not have to postulate a list of meanings. Pustejovsky's lexicon is generative in the sense that meanings of polysemes arise from context. Fodor and Lepore (1998) in their best tradition attack Pustejovsky's hypothesis from the position of conceptual atomism.

In this section I propose to start with a brief exposition of Pustejovsky's views, and then proceed to Fodor and Lepore's objections in the next section. However, Pustejovsky himself distinguishes between logical polysemy, which can be generated within his lexicon and "more general operations of sense transfer such as metaphor, etc., which generative lexicon theory claims are extralinguistic transfer phenomena" (Pustejovsky and Boguraev 1996: 9). One is asked to keep in mind, therefore, that polysemous adjectives Pustejovsky deals with are of the "good" and "fast" kind (literally polysemous adjectives), not of the "bright" and

"sharp" kind (metaphorically polysemous adjectives), and any mention of the latter kind in what follows below will be a reconstruction, not a reproduction of Pustejovsky's hypothesis.

Pustejovsky suggests that each lexical item is represented in the mental lexicon with its qualia structure which, together with compositional rules, contributes to the "contextual determination of an expression's meaning" (Pustejovsky 1998: 289). There are four levels of representation for each lexical item: argument structure, event structure, qualia structure and lexical inheritance structure. Qualia structure specifies a lexical item's qualia roles, including its formal, constitutive, telic and agentive roles. An example will remind the reader of Pustejovsky's idea of how the lexicon is organized (see ibid.).

Thus, for the lexical item "book" we have a representation in the mental lexicon which should tell us that "book" can refer both to a physical object and information (this is the argument structure of the concept 'book'). The event structure for this item is absent because "book", unlike "examination" or "lunch" does not refer to a process. The qualia structure of the concept 'book' specifies its formal role (books qua physical objects hold books qua information)[95]. Its telic role or its function, the purpose of its existence is "to be read", which we need to specify in the lexicon in order to say that the default interpretation of "enjoy the book" is "enjoy reading the book" (this provoked a number of criticisms, since "enjoy the book" may be as much "enjoy writing the book" as "enjoy reading the book"). Finally, the agentive role contains information that books are written by somebody (this qualia role is specifiable only for artefacts).

The introduction of qualia structures into representations of nouns was supposed to solve the problem of the productive polysemy of verbs and adjectives[96]: when we want to know what "enjoy" means in a particular context, we go looking into the telic role of the noun whose referent is being enjoyed. With respect to adjectives, we get a phenomenon of co-composition: we do not assume that "a fast typist" is someone who is a typist and who is fast, but look in the qualia structure of 'typist' to find out what is the most prominent activity of typists, since "fast" pertains to rates of processes. And thus we arrive at the interpretation of "a fast typist" as "someone who types fast" (see Jackendoff 1998: 62-63). Generally, what is achieved by this is "capturing" the creative use of words through rich lexical representations: "the same expression can serve countless purposes *because* the semantic features (meanings) change in context" (Pustejovsky 1998: 291).

---

[95] For the purposes of simplicity I do not reproduce here Pustejovsky's schematic representations of qualia structures, which in the present case would be FORM = hold (x, y), where y is ARG1 = information and x is ARG2 = phys-obj. For detailed formalisms the reader is referred to Pustejovsky (1995, 1998). I, however, tend to think that verbal exposition will make it clearer what the point of Fodor and Lepore's critique is.

[96] Noun polysemy is resolved within the representations themselves (argument structure).

Even though Pustejovsky claims to have reconsidered the framework of lexical semantics by introducing into the lexicon generative devices instead of conceptual primitives (ibid.: 293), the whole story is more like putting new wine into old bottles. Concepts (entries in the mental lexicon) remain complex, and the meanings of words definitional. 'Book' is individuated not by its extension, but by a number of inferences one is required to make about books, generating those from the qualia structure of the concept. The old principle of dividing meanings into basic and derived (ibid.: 304) is still there, and particularly evident in Jackendoff's (1998) discussion of polysemous adjectives.

As mentioned above, Jackendoff believes that one cannot apply the formal semantic approach to the analyses of polysemes such as "fast", "sad" and "good". But the true reason for requiring a new analysis is not that if you look for an overlap between all fast things and all typist-like things you will not acquire the meaning of "a fast typist", but rather that "fast" has a basic (or a primary) meaning, which applies to the rate of a process (as in "fast waltz"). Therefore, you cannot simply apply "fast" in its primary meaning to a typist or a road since neither is a process. The qualia structures are needed to tell you what processes are associated with the two lexical items: typists by default type (thus, "a fast typist" is someone who types fast); roads are objects over which one travels (thus, "a fast road" is a road over which one travels fast). So, everything hinges on "fast" having just one basic meaning and the reader is not told anywhere how the basic meanings are to be determined.[97]

Jackendoff claims that something similar is going on with "sad":

> "*sad* pertains to a person's emotional experience. It therefore can modify *woman* by simple composition, but not *event* or *movie*. Rather it must pertain to the emotional experience of characters found in the qualia structure of *event* and *movie*..." (p. 63)

So, the qualia structure of 'event' should specify that events have participants, and the qualia structure of 'movie' that movies are viewed by people. However, this will not resolve all of the ambiguity: if "a sad movie" is to be understood as a movie that makes one experience sadness or makes one feel sad, we still have to find out whether it makes one experience sadness as one's personal experience of sadness or whether it makes one feel sad on someone else's behalf. (After watching a war documentary one may perfectly well utter a detached "It's a sad world", but not experience sadness at heart; feeling sad for other people is different from feeling sad for personal reasons). The need for refinements may go *ad*

[97] There is a standard objection to this example, which shows that the interpretation suggested by the qualia structure can be overridden. In the context "The fast typist won the race", the interpretation of "a fast typist" is not "someone who types fast", but rather "someone who runs fast". However, one

infinitum, but it is not clear how much of them the lexicon is required to capture and under what criteria.

Moreover, if Pustejovsky's generative lexicon is to be taken not as an abstract semantic theory, but as a psychological hypothesis concerning conceptual organization, where qualia structures are parts of semantic representations of concepts (which it well may be – see Pustejovsky 1995: 19) then it raises a number of questions. If the interpretation of "a sad event" or "a sad movie" were indeed more psychologically complex than the interpretation of "a sad woman" (as entailed by their analysis), then it would take longer to process the former two expressions than the latter one, and that would be reflected in psycholinguistic experiments. (However, even metaphor processing is no more time-consuming than the processing of presumably literal meanings – see, for instance, Gibbs 1992).

Thus, Pustejovsky-Jackendoff's analysis of polysemous adjectives does not take us much further from the starting point: if "sad" is as immediately interpretable in "a sad movie" as it is in "a sad woman", and if in some cases it means "to experience sadness" and in others "to cause sadness", then the qualia structures are of no use for resolving polysemy into a single "entry", and "sad" remains ambiguous much as it is in the enumerational lexicon: it means "to experience sadness" in some contexts and "to cause sadness" in other contexts. If one wanted to show that "sad" is not polysemous, one would have to stipulate that the meaning of "sad" is the same in "a sad woman" and "a sad movie", that is, that there is only one underived meaning (a single concept in the mental lexicon) which contributes to the interpretation of all "sad"-instances. To speak of basic and derived meanings pushes ambiguity further in instead of eliminating it.

One can see now why lexical semanticists in this tradition avoid the question of "metaphorical" polysemy, claiming that it belongs not to the properties of the generative lexicon, but to some extralinguistic transfer capacities. Consider those polysemes that I have been discussing all along: "sharp" and "bright". Unlike "fast" and "sad", whose usage across sensory domains is never perhaps metaphorical ("a sad light" is "a light that causes sadness", "a sad remark" is "a remark which expresses sadness"), "sharp" and "bright" behave differently. One cannot apply to them the generative lexicon analysis because adjectival polysemy is supposed to be resolvable not within the adjectives themselves, as it were, but through the qualia structures of those nouns with which they compose.

It is clear that one cannot expect the interpretation of "bright music" or "sharp tongue" to be generated by Pustejovsky's lexicon, since the interpretation of the adjectives in question

---

cannot expect the qualia structure to specify in the qualia structure of 'typist' all those things that typists may happen to be doing when they are not typing.

should be constrained by the qualia structures of the nouns "music" and "tongue", the adjectives themselves being univocal across semantic fields. As we have seen above, the supposed univocality is only superficial: according to Pustejovsky and Jackendoff, there are after all basic meanings. So, the basic meaning of "bright" pertains to visual stimuli and the basic meaning of "sharp" pertains to cutting and piercing objects. Music is not a visual stimulus, tongues are not normally considered to be cutting objects. But, if in the case of "a fast typist" and "a sad movie" we can find a rule of composition (the function of typists is to type; the function of movies is to have an effect on their viewers), there are none that would similarly generate the interpretations of "bright music" and "sharp tongue" (although, as with 'movie', one could stipulate that the function of music is to have an effect on its listeners). Thus, the literal-metaphorical distinction is retained in its full force in Pustejovsky's semantics.

However, the cases of adjectival polysemy I deal with here are interesting not only semantically (most of them are part of the lexicon and their dictionary entries are not even marked "figurative"), but also psychologically (see part II). Pustejovsky's apparatus offers no devices to treat them, and moreover suffers from its own internal inconsistencies (its psychological unsoundness and the lack of meaning/ concept univocality which the theory was supposed to arrive at). Therefore, the generative lexicon story cannot be the theory of concepts corresponding to "metaphorically" polysemous adjectives.

6. 4. 3. Fodor and Lepore's critique of Pustejovsky.

In this section, I will first reproduce some of the critique Fodor and Lepore directed against Pustejovky's theory of generative lexicon, and then discuss the disquotational lexicon they offer as an alternative to see if it solves the problem of polysemy. The essence of Fodor and Lepore's argument is that lexical meaning is atomic and should be identified with denotation as opposed to Pustejovsky's idea of complex lexical entries which have to specify the inferences one is justified in making. Since the issue of meaning holism has been considered above, I will concentrate here on some particular examples of verbal and adjectival polysemy that Fodor and Lepore discuss.

One such example is "bake" which, according to Pustejovsky, "is polysemous between 'a creative activity' as in *bake a cake* and a 'change-of-state predicate' as in *bake a potato*" (Fodor and Lepore 1998: 278). In line with his major postulate that the meanings of polysemes can be generated from some basic meaning plus the contribution of the

expressions with which the word (concept) is being combined, Pustejovsky suggests that the creative reading of "bake" in "bake a cake" is contributed by the qualia structure of "cake", which is an artefact. "Potato" refers to a natural kind and cannot be produced by baking, therefore "bake a potato" receives the "change-of-state" reading. However, as Fodor and Lepore note, the story is not all that simple. First, "bake a cake" is itself ambiguous between a creative activity and heating up (frozen foods cakes). Second, being an artefact cannot justify a creative reading in itself: "to bake a knife" and "to bake a trolley car" are resistant to it. Thus, they conclude that "bake" is lexically ambiguous and not reducible to just one basic meaning.

Another example is "good" (see also above in 6.4.1). According to Pustejovsky, "good" gets its meaning from the telic functions of nouns with which it is being combined: if it is "a good knife", then "good" should be read as "good for cutting", etc. This analysis is again problematic, since it presupposes that all objects and entities referred to by nouns have functions. However, "good weather" and "good children" are perfectly plausible, although neither weather nor children have clearly delineated functions (or any functions at all) that could be specified in the qualia structures of these concepts. According to Fodor and Lepore, "good" is context insensitive and "a good NP" always means "one that is good for *whatever it is* that NP's are supposed to be good for" (ibid.: 286)[98].

So, this is Fodor and Lepore's general procedure: they show that all of Pustejovsky's examples either do not avoid the issue of ambiguity or do not really appear to be context sensitive and depend on the qualia structures of the nouns with which they combine. Thus, according to their story, there are cases of real polysemy as exhibited by such lexical items as "bake", which is ambiguous between two meanings. The two meanings refer to different processes and this is compatible with denotational semantics (in order to understand which process is being referred to you disambiguate sentences). But there are also lexical items which remain unchanged in all contexts. They include such items as "good" and "enjoy", which always mean 'good' and 'enjoy' whatever it is that you find good or enjoy.

Thus, lexical meanings are atomic (not complex) and specify denotations. However, to resolve the situation with such cases as "lamb" (as meat and as an animal), "window" (as the opening and what fills the opening), "newspaper" (as the thing that people read and the organization that produces it), lexical meaning has to specify the rules of composition which help determine the logical form of those phrases where a certain lexical item figures (if you

[98] Fodor and Lepore's view *is* different from Pustejovsky's view in that "good" does not presuppose that a noun it is being combined with has a particular function. However, the way they put it – "NPs are supposed to be good for" – makes one suspect that probably deep down their view is not much different, since "being good for" has itself functional connotations.

read a newspaper, it is clear that in your context "newspaper" cannot refer to the publishing organization). This notion of logical form is also useful in explaining why from "want a beer" we can reasonably infer "want to have a beer" (the logical form is "want an NP") and why "The woman ate" is well-formed unlike "The woman devoured" (thus rejecting semantic or content considerations that may be applied to explaining the difference – see ibid.: 277).

Now, we have arrived at the point that interests me here most: how do we deal with adjectival polysemy of the synesthetic and double-function types? As we remember, Pustejovsky's generative lexicon excludes such items from consideration, claiming that they belong to extralinguistic factors. In this respect, what Fodor and Lepore say is not very different. Commenting upon such cases as "window" and "lamb", they write:

> "We suspect that there is nothing to say about such cases; the meanings of words can partially overlap in all sorts of ways in which polysemous terms can differ from mere homonyms." [continued from a footnote] "There is a semiproductive generalization according to which terms for tastes double as terms for personalities: *sweet, bitter, sour, tart, acid, bland, salty,* and so on. Could anyone really suppose that lexical semantics should be required to capture this regularity? And if not, why should it be required to capture the polysemy of *window, door, newspaper*, and the like?" (p. 287)

And thus, they conclude the article saying that polysemy should not be a problem for lexical semantics, but that compositionality should. Namely, it is up to semantics to find out what being compositional amounts to and in what ways lexical items may constrain each other in the context.

For Pustejovsky, such terms had to be excluded from the lexicon as they constituted an irremediable problem for the qualia structure (it is difficult to imagine what could have been in the qualia structure for 'girl', if we had expected it to contextually modify the meaning of "sweet")[99]. For Fodor and Lepore, the story is slightly different. Since meanings are denotations, they are supposed to pick out in the world only those sets of objects, or sets of properties, and sets of relations that can be literally covered by a word/ concept. "Window" is not polysemous[100] since windows as openings and windows as those things that fill the openings usually come together in the real world, and which part of them one is referring to becomes clear from the logical form of the expression where "window" is found. Similarly,

---

[99] I cannot resist the temptation of quoting from Fodor (1998a) at this point: "If the examples work, they [lexical semanticists] count them for their theory. If they don't work, they count them as metaphorical extensions." (p. 59)

[100] Or, more precisely, "window" is not polysemous in Pustejovsky's sense, i.e., the lexicon does not have to specify the source of the polysemy. One could say that for Fodor and Lepore, "window" is an instance of polysemy without simple ambiguity, where distinct meanings can nevertheless be similar.

for "good" and "enjoy", which are not polysemous because their meaning does not change from context to context, being simply 'good' and 'enjoy'.

But other terms turn out to be ambiguous, i.e. there are ['term1'], ['term2'], etc. in the lexicon, and the same word in a language by some accident corresponds to several concepts. Such an example is "bake", which is polysemous between creating something by baking it and warming something up by baking it. Therefore, it requires disambiguation in the context: when you encounter "Mary baked a cake", you have to map the "bake" you hear either to 'bake1' or 'bake2' in your mental lexicon (and by Fodor and Lepore's account, there is nothing to show that the two are connected in any other way apart from having the same name in the language). However, this is somewhat inconsistent with what Fodor (1998a) said about "keep". Although at one point he hesitated as to whether "keep" was just polysemous or ambiguous as well ("He kept his promises and his snowshoes in the cellar", p. 50), in the end he came to viewing "keep" as univocal across domains, i.e., "keep" always triggers the same concept 'keep' and always refers to instances of keeping (see section 6.4.1).

This no-polysemy view goes well in hand with semantic atomism: 'keep' is an atom, and the reason we use "keep" in a variety of contexts is only that in all of these instances we depict the same relation in the world, which is precisely the relation of keeping. Thus, here Fodor rejects polysemy to make sure concepts are not definitional or decompositional: "keep" does not mean 'the causation of a state which endures over a period of time' and does not reduce to any meaning constituting primitives, being a primitive itself. Fodor and Lepore's "bake" proves to be different. It is not the case that in "Mary baked a cake" and "Mary baked a potato" the word "bake" has 'bake' as its meaning or that it refers to the same instance of baking. And here, it is only a half-step away from saying that the meaning of "bake" is given by definitions: the standard story that philosophers tell (see Fodor 1998a: 69-87).

The only straw to keep Fodor and Lepore away from slipping into meanings as definitions is to insist that the meanings of "bake1" and "bake2" overlap only accidentally. That is, that the meaning of "bake" in "bake a cake" is 'bake1' which refers to the activity of making something by baking, but which does not mean "to make something by performing such and such operations with it to which we normally refer as 'baking'". Similarly, for "bake" in "bake a potato" and 'bake2'. Thus, in case of ambiguous terms such as "bake", we avoid saying that meanings are definitions (we disambiguate between "bake" as 'create' and "bake" as 'warm up') only by postulating *a priori* that meanings/ concepts

are atomic and that concepts that are named by the same names are named by them as a matter of chance only, without any structural or functional similarities.

This puts "bake" on the same row with "bank". The last bit of their *a priori* requirements is, however, problematic. "Bank" as in "the bank of a river" and "bank" as "the place where you keep money" have little in common, while the two instances of baking overlap at least in that when you bake something you normally shove this something into the oven, no matter what its origins are (creative or non-creative). Thus, even though Fodor and Lepore want to keep polysemy and true homonymy apart (p. 281), their assumption that the meanings of polysemes are not connected in any interesting way will not let them do so. Therefore, they either have to deny polysemy ("window") or to reduce it to a special case of homonymy (the word "bake" expresses two different and unrelated concepts). What is being missed out is the systematicity that is found in polysemy and is lacking in homonymy (see also Pustejovsky 1998: 308).

Let us look at the question from the other side: what is to prevent one from saying that "bake" is not polysemous, that "bake" always means... 'bake'? Consider: there is nothing pleonastic in the sentences "Mary baked a cake she made" and "Mary baked a cake she bought". In both these sentences "bake" means exactly 'bake', where "bake" refers not to a creative or a warming up activity, but rather to the activity of transforming an uncooked object into a cooked object by placing it into devices invented for baking (ovens, for instance) and keeping it there for a necessary amount of time. Fodor and Lepore accuse Pustejovsky of wanting to introduce ontological information into the lexicon without providing an argument for why it should be there (p. 281). However, they are tripping over the same little problem: they introduce world knowledge about things that can be baked into the meaning of "bake". But even the denotational semantics cannot require that concepts pick out all or some of the activity preceding an instance of a certain action-type (i.e., that 'baking' should be about anything else but shoving an object into the oven)[101].

Thus, Fodor is being caught between atomism, which requires conceptual identity, and "enumerationalism" (or strict compositionality), which requires that the meanings of polysemes be kept apart unless they are completely context invariant. This leads to the inconsistencies we have observed above. It should be clear by now that the second requirement prohibits introducing the polysemy of "sweet" and "bitter", etc. into the lexicon

[101] The difference between "baking a cake" and "baking a trolley" is not a difference that can be specified in the lexicon – it has to be supplied by our world knowledge. It may be reflected grammatically in the difference between the perfective and the imperfective. There is nothing wrong semantically with "I am baking a trolley" - it refers to a particular activity in the real world, however nonsensical this activity may appear to us. And the reason it appears nonsensical to us is that on the basis of our world knowledge we realize that "I have baked a trolley" may never come true.

but for reasons different from those of Pustejovsky. His semantics requires that the meaning of a term (bar indexicals) be context invariant, e.g. that "sweet" always refer to the same property in the real world, and its meaning be given by the mind-world relations. Thus, "sweet" in the context of cakes and cookies is only accidentally and merely as a matter of the name related to "sweet" in the context of personalities: whatever we may happen to have in the lexicon is going to be of the type 'sweet1', 'sweet2', etc. But if we leave aside the rhetoric from compositionality, what all this boils down to is that there are primary literal meanings (however we happen to establish them), which are in the lexicon and "metaphorical extensions", which are a matter of anything you like but semantics (and this brings informational semantics close to lexical semantics: all the trouble is caused by having basic meanings). And thus, we are back to Davidson (1978): the message a metaphor conveys (presumably, also metaphorical types of polysemy convey) exists only in the imagination of the interpreter.

Even if there are cases where this view of metaphor is indeed true, there is something striking about the types of polysemy I have been discussing, especially synesthetic expressions: they are not only found cross-culturally and have fixed dictionary entries, their univocal interpretation is available across populations varying in age and mental abilities. So, here is the rub. It is all right when we talk about semantics as a theory of meaning *in abstractia* to suggest that polysemes be better kept apart to make them available for procedures of verification or falsification. However, if informational semantics (informational atomism) of Fodor's type is supposed to be a theory of concepts and content of thoughts, not a theory of how to compute meanings which are not attached to thoughts, then Fodor and Lepore's proposal won't do. As Fodor (1998a) mentioned, psychological research shows that meanings are unlikely to be definitional (see Carey 1982). In the preceding chapters, I have presented more than sufficient evidence intended to show that the meanings of polysemes (polysemous adjectives) are related in ways that are far from being accidental.

Thus, there are two moments which have to be resolved: the problem of compositionality and the problem of literalness. As to the latter, I have already expressed my doubts concerning the lines that different theorists draw between the literal and the metaphorical, showing that for a large part the assumption of primary literal meanings is a matter of an *a priori* semantic construal rather than uniquely available empirical evidence. As to the former, there are also reasons to doubt that what we need in semantics is the notion of strict compositionality (full context independence). Fodor and Lepore conclude that since the rigorous notion of compositionality cannot be had (p. 287), the lexicon must specify not

only denotations but also the logical form. If language is compositional, it is because thought is compositional (Fodor 1998a: 26), but Fodor and Lepore's denunciation of polysemy does not contribute to making the notion of the compositionality of thought any clearer.

So, in the next chapter I will attempt to present a hypothesis about concepts which does not part with polysemy as a mere impediment, but which nonetheless retains the ideas of conceptual atomism, meanings as mind-world relations and a non-rigorous notion of compositionality. The price of having all these things together is that the enumerational lexicon as a theory of mental content and the assumption of literal meanings will have to be reconsidered and much of it will have to go. In the preceding chapters, I have been criticizing non-philosophical theories of concepts from the point of view of their philosophical viability. In the next chapter, I will have to show how and whether my no-polysemy hypothesis can withstand a philosophical critique.

Chapter 7. Espousing the no-polysemy view.

7. 1. Clearing the ground for a new no-polysemy view.

I finished the last chapter by stating that Fodor's views on polysemy proved inconsistent, and that the putative no-polysemy view he expressed in his 1998 book turned out to hinge on ambiguity. Therefore, in the framework of informational semantics the problem of polysemy has not been resolved and the different meanings of polysemes are kept disconnected, in contradiction with empirical evidence for their interconnectedness in the speakers' mind. In the present chapter, after a brief reminder of what has been happening before, I will attempt to present a no-polysemy view which does not appeal to ambiguity and which is consistent with the psychological data.

The story begins with cognitive linguistics' provocative insight that the meanings of words overlap not in any accidental but a highly systematic way which was labelled metaphorical projection (think of the vast number of actions to which we refer to as "arriving"). It was shown that the psychological and philosophical assumptions underlying cognitive linguistics' research are not viable. However, the observation that we apply the same terms (and thus, probably employ the same concepts) to a variety of conceptual domains remains unchallenged. The same insight guides much of the research in lexical semantics, which intends to show how meanings are generated in context, given the common conceptual core that underlies the usages of a term across domains. In the preceding chapter I pointed out the flaws this research suffers from.

The narrow area of my interest here was polysemous adjectives of the metaphorical type: adjectives spanning several sensory modalities and adjectives covering physical and psychological properties. I started with considering the overlap between "hot" (to taste) and "hot" (temperature sensation) in English. It turned out that other languages exhibit the same pattern, associating the sensation produced by spicy foods with other potentially painful sensations (and the word for spicy food sensations corresponds either to the morphemes for "hot" or the morphemes for "sharp", "pricking"). There is also physiological evidence that the ability to bring together the two seemingly different sensations lies in the properties of nociceptors, or pain-detecting mechanisms. However, it cannot be the case that the conceptual structure resides in physiology: this would leave unexplained the difference in patterning (not all languages use exactly one and the same morpheme for spicy taste sensations) and dispel linguistic conventionality (for it is conventionality that we have to appeal to when trying to explain why, for instance, in Russian "sharp taste" means "hot

taste", but the English "sharp taste" means something different, although not disallowing completely the "hot taste" interpretation; for details see chapter 2).

The next step was to consider synesthetic expressions of the type where an adjective which is considered properly to belong to one sensory domain is combined with a noun from a different domain (e.g., "dark voice"). There, it became even clearer that mental content is not fully reducible to the physical: although from early infancy people possess the physiological and psychological ability to correlate information from different sensory modalities (bright lights resemble loud music in their stimulus intensity), this is not sufficient grounds for determining the meaning of synesthetic adjectives. Thus, under the view that reduces conceptual organization (conceptual space) to perceptual organization (perceptual space) "bright music" and "loud music" should be considered equivalent expressions. However, they are not synonymous, and they are not coextensive: bright music can be played softly (see chapter 3 for details). Therefore, something different is going on here from an analogical comparison[102] between the domains of vision and audition.

I further claimed that there is no qualitative difference between synesthetic and double-function terms and that the explanation for them should be sought on the same grounds (this claim is confirmed by research on brain-damaged populations). The way "physical" adjectives double as "psychological" adjectives is striking: evidence of this are cross-linguistic studies showing the widespread nature of the phenomenon and psychological research, including developmental, psycholinguistic and clinical studies. And here we again come across a puzzle: there is no explanation currently available which would do the job of clarifying how the purportedly physical terms acquire their psychological meanings (for more details concerning the discussion to follow below see chapters 4-6; here I am only reiterating). When we talk about bright and dull people (as people in many linguistic communities do), we have no trouble understanding the intended meaning: indeed, in many languages the corresponding morphemes have psychological meanings as parts of their dictionary entries. However, there is no simple similarity between bright people and bright lights, nor is there an analogical similarity between them.

It's clear why a simple similarity relation (as in "She's got spaghetti hair') is lacking here: however long one may look at bright visual objects (the sun, for example), one will not see the properties that could be transferred to psychological qualities of persons. The

---

[102] By analogical comparison I mean comparison involving at least four terms: x:y:: t:z (loud: soft:: bright:dim). Non-analogical comparison involves only two terms, like the comparison of a light's brightness with a piece of music's loudness, which can be done unconsciously. If a metaphorical transfer based on analogical comparison was the basis for deriving the alternative meanings of cross-modal adjectives, it would lead to the coextensionality of the terms in the formula, which, however, does not happen.

analogical similarity explanation (found, for instance, in cognitive linguistics) is more tricky: why cannot one say that we compare bright people to bright lights invoking the whole continuum of dull/dim to bright? There are two objections to this view. First, any analogical explanation of this type presupposes the unquestioned validity of evaluative syllogisms (intelligent people are more pleasant than ignorant people, bright lights are more pleasant than dull lights, therefore intelligent people may be compared to bright lights, and the term "bright" may come to mean 'intelligent'). However, "a bright person" is not merely an evaluative judgement, or rather it is something quite different from an evaluative judgement, for it is perfectly possible to say: "He may be a bright person, but he is an idiot" (for more examples see chapters 4-5).

Second, and more important, any such explanation depends heavily on the assumption of meaning holism. It presupposes that the meaning of "bright" can be given only relative to the meaning of "dull" (or "dim", or any other term that one happens to think relevant). In the preceding chapters I argued extensively that it is not necessary that the meanings of sensory (or any other) terms be holistic. The possession of corresponding concepts ('bright', 'hot', etc.) is given through mind-world relations. And the reason to take mind-world relations as meaning-constitutive is that, for instance, cross-modal associations are not just a matter of linguistic juxtaposition but a real psychological phenomenon. To correlate loud sounds and bright lights one does not need to know whether there are other intensities in either modality. Similarly, one does not need to have the concept 'quiet' in order to have the concept 'loud'[103].

There is still another possibility that deserves attention. Since most double-function terms have their psychological meanings as parts of dictionary entries, why cannot we consider them as merely ambiguous, and start looking for ways of contextually disambiguating them instead of worrying whether they are systematically polysemous or not. The reply to that is almost self-evident. Above I showed that the systematic polysemy of double-function terms is found not only in English, but in other, unrelated languages as well. Now, imagine a language which has a fixed psychological entry for its "bright" morpheme, but does not have any such entry for its "dull" morpheme (which is the case in Russian, for instance). Given that upon encountering the "dull" morpheme in a context

[103] To reiterate what was argued in chapter 5: We can imagine a community who can only detect those sounds that are loud to us or whose environment only subjects them to auditory stimuli which we would label "loud". They are likely to have in their mental lexicon the concept 'loud' but not the concept 'quiet'. One may argue that they mean something different when they say "loud" from what we mean when we say "loud", because "loud" for them is the only term for the auditory modality. However, this is an illusion created by our being so used to the contrast "loud"-"quiet". If they are

similar to "He's such a dull person" the speakers of that language have no difficulty understanding the intended meaning and interpret the sentence univocally (this suggestion was informally tested for native Russian speakers), polysemy strongly appears to be a real question for semantics.

Similar situations may be discovered even without leaving the boundaries of English. The auditory or visual synesthetic meanings of "cold" are not parts of dictionary entries (at least of COED, since neither "cold light" nor "cold music" fall immediately under the definitions "lacking ardour, friendliness, or affection" or "depressing, dispiriting, uninteresting"). However, the two expressions are readily interpretable (and are such even for people with reduced mental capacities). What is more, we would not be able to reduce them to definitions (see previous chapter). Therefore, the possibility that the terms "dull" and "cold" are merely ambiguous is ruled out since what we have to disambiguate between is not given *a priori*. And the reason I mentioned this possibility is that the disambiguation story as a theory of meaning may do for time slices of languages (those historical periods for which we can stipulate the existence of a definite number of meanings of a word). Nevertheless, if we want to have a theory of concepts, we have to possess the apparatus which would allow us to explain the ability to univocally interpret synesthetic and double-function adjectives in novel contexts[104].

One more remark before I proceed to the no-polysemy hypothesis. All the theories discussed above, despite a large number of disagreements between them, share one major assumption, which is the assumption of literal meanings. Because of this, any attempt to explain the polysemy of synesthetic and double-function words runs up against essentially the same barrier. Cognitive linguistics assumes that only those meanings that are grounded in the most immediate physical experience can be basic or literal, and the rest should be their metaphorical derivatives. However, as shown in chapter 1, in order to derive certain schemata from experience one should already possess in-built capacities for schemata detection. Lexical semantics holds that some meanings are primary and are such in order to make them fit lexical semantics theories. We saw above why synesthetic and double-function terms do not fit. The putative theory of meaning of Marks et al. offers no grounds

---

tuned to the world in the same way as we are, the content of their concept 'loud' will be exactly the same as ours, and they will apply "loud" to the same stimuli as we will. For more of this see ch. 3.

[104] The impression that I stress here a difference between theories of meaning and theories of concepts is the right one. Theories of meaning may deal with language outside of speakers (for instance, consider polysemous terms as merely ambiguous). Theories of concepts cannot. If, in understanding and interpreting someone's utterance, the interpreter maps the words occurring in it onto concepts (following the rules of syntactic composition), a theory of concepts must be able to provide an account of what concepts are being employed and how. "Enumerational" theories of

for distinguishing between the literal and the non-literal apart from saying that some terms are proper terms of some modalities and not others. Finally, informational semantics insists that only some meanings of terms may be considered literal (and within the lexicon) in order to preserve their referential function. As we saw above, its treatment of polysemy is not consistent: we may preserve the unity of denotation by separating senses to map different concepts; however, this runs contrary to the thesis that words express concepts and refer to the same type of relation in the external world (the "keep" example).

7. 2. The no-polysemy view and conceptual structure.

As with the theories I discussed, there are also some initial assumptions that the no-polysemy view is built upon: 1) the denial of the literal-metaphorical distinction with regard to conceptual structure and, as a result of it, the denial of the literal-metaphorical distinction with regard to language (note, however, that the present discussion is only concerned with some types of adjectival polysemy and corresponding concepts) and 2) the claim that a theory of concepts does not have to be at the same time a theory of meaning.

The concepts 'bright', 'sharp', 'cold', etc. are psychological primitives spanning all domains of human experience (perceptual modalities, introspection, interpersonal relations, etc.). Their content cannot be further decomposed into features which would be more primitive than them: there is no core feature which would be available across domains plus an inexpressible little bit to distinguish them across domains as some versions of lexical semantics want to have it. Concepts corresponding to polysemous adjectives are irreducible and "indivisible". One may want to think of those psychological primitives as neuronal configurations responding to a range of stimuli (if it is hard to imagine 'bright' as a primitive concept, being used to the idea that "bright" can only be properly applied to the visual modality, one can think of it as a primitive concept 'B', about which we know only that it becomes activated, or may become activated, in the presence of certain visual, auditory, etc. stimuli).

However, the content of psychologically primitive concepts is not reducible to physiological and/or perceptual functioning (if it did, this would have lead to "bright" in "bright music" being synonymous with "loud"). They are primitives in the representational system, built-in devices for categorizing stimuli coming from the environment through

---

concepts (which have it that there are unrelated concepts 'cold1', 'cold2', etc.) are not compatible with the psychological evidence (see chapter 5).

different modalities. Recall Fodor's (1998a) analysis of "keep": "keep" is not polysemous because in all its instances it corresponds (or is being mapped) to the same concept 'keep'; in all the situations to which we refer employing "keep" we detect the same relation of keeping. I suggest that the same analysis be applied to polysemous adjectives. Thus, there is a single concept 'bright', or a single concept 'sharp', which we employ whenever we come across instances of brightness or sharpness in our experience, linguistic or otherwise. That is, we act as good brightness or sharpness detecting mechanisms and are able to notice the presence of the same property in a variety of instances.

The last point requires some clarification. It might be much easier to accept 'sharp' as an amodal concept than 'bright'. "Sharp", after all, sounds perfectly literal when predicated of knives, or sounds, or lights. "Sharp" is something that irritates one by its intensity (à la Fodor: its *sharpness*). "Bright" is different: only objects in the visual modality can be literally labelled "bright". Such reluctance is ill-grounded, however. A blunder one is committing when offering such an objection is confusing words with concepts. To a large extent, it is a question of linguistic conventions what terms are considered to be properly predicating what properties, and conventions are subject to change. Note also the role that the word "properly" (and the like) is playing in all such views (for instance, see the discussion of Marks et al. in chapter 3): it sounds more like an ethical judgement than a rigorously defined criterion of discrimination.

If one leaves "properly" out, the objection appears less impressive: that the term "bright" predicates only a certain property in the visual modality is not even true. Speakers may employ "bright" referring to properties in other modalities (for instance, "bright voice"). Unless one is prepared to say that they are not doing it "properly", the objection hangs exclusively on an a priori assumption that there are literal and non-literal meanings. Now, concepts are different: they are not conventionalized, they are what we have as a result of our evolution (perhaps, also our social situation, etc.). The fact demonstrated in psychophysical experiments, namely that humans, when prompted by "bright", are able to classify auditory stimuli (not auditory linguistic stimuli) into falling or not falling under it allows one to suppose that 'bright' is a concept that is not tied up with the visual modality to the exclusion of others.

Moreover, one should not interpret this as the simple similarity view, according to which it would be the perceptual ability to correlate stimuli across different modalities that led to conceptual overlaps. If anything, the view I am expounding should be called the reversed similarity view: with respect to polysemous adjectives representations come before

perceptions[105]. That is, 'bright' is a psychologically primitive concept, which serves to sort out stimuli into categories (of 'bright' and 'not bright') whatever the source of incoming perceptual information. This is the reason why sounds or smiles can be judged bright or not: not because they remind one somehow of lights and become correlated with them, but because 'bright' is a representational primitive. As such it has to be atomic and innate. It is atomic for the reasons largely discussed above, namely, because the brightness of a light or a sound is constituted by causal mind-world relations, not by semantic relations between "brightness" and "dimness". And it has to be innate if we want polysemy to be grounded in the conceptual system (and also because abstractionism will not do the job: from looking at bright visual objects it is a long way to having the concept "bright" – see Geach 1957: ch.6).

One may object that even granted that 'bright' and the like are representational primitives, this does not answer the question of how sounds may be judged bright or not. However, this is not the aim of my thesis. It is intended to show that there is a principled possibility for different meanings of polysemous adjectives to be grounded in the same concept. How the connection between properties across modalities is made possible is open to empirical research. Nevertheless, empirical evidence presently available suggests that such connections are indeed being made. Cytowic' (1989) experiments showed that non-synesthetic subjects make reliable connections between shapes and tastes. Marks et al.'s work shows that predicates are universally translatable across modalities. Asch's work shows that there are more similarities than differences in the way people in different linguistic communities associate psychological qualities with "physical" adjectives. It also shows that children master the psychological meanings of polysemous terms without appeal to their physical meanings. Research on brain-damaged subjects shows that they are able to interpret instances of metaphorical polysemy even though they may judge them unacceptable.

In a sense, asking why we are able to detect brightness or sharpness across experiences is akin to asking why the ability to detect verticality should be present innately. The answer to the latter will not be because there are plenty of vertical lines in our environment, but rather because the ability has by now become pre-wired (see Young 1987: 123). The ability to detect verticality and horizontality is one example of what representational systems of

[105] It would be justifiable to wonder how such representations could have evolved. There is no space to go into this in detail here. Roughly, what I mean is that such representations are not formed in the course of individual development but are overall human heritage, and that they appeared at whatever time it was when we can distinguish between non-human and human conceptual abilities. According to Cytowic's suggestion (see section 3.5.4), it is the changes in the mechanisms of sensory integration that brought about a new evolutionary state.

animals incorporate. The human representational system (the conceptual system)[106] is a continuation of the same ability to organize instances of experience according to schemata of perception. I suggest that 'bright' and 'sharp' are two examples of such schemata, which are themselves amodal, but which have grounding in perceptual experience, even though they are not derived from it (thus, the view I am proposing is not susceptible of the criticism of amodal conceptual systems expressed in Barsalou (1999)[107]).

The difference between humans and animals that is, naturally, of interest to us here, is that the former possess language which is productive and compositional. Granted that 'bright' and 'sharp' are representational primitives, or psychologically primitive concepts, there is still a question of how they become realized in linguistic functioning. Namely, why some of their applications seem literal while others do not. I suppose, one way to answer this is to say that some properties become more important for survival purposes and thus acquire linguistic stability (in the sense of the presence of which properties it is more important for one to communicate successfully to the members of the same community; compare also your intuitions about the literal meanings of "bright" and "sharp": "bright" seems literal only when predicated of objects in the visual modality, "sharp" seems to have more than one literal meaning: "sharp knife", "sharp pain", "sharp sound"). However, this is not to show that the difference between the literal and the metaphorical meanings of polysemous adjectives is an *a priori* matter.

First, we have the question of those meanings that acquired their dictionary status (recall that according to the OED "bright" as of sounds in not marked *figurative*). If we have a fixed meaning for a polysemous adjective whose first meaning we consider to be different, what does saying that this other meaning is not literal amount to? Since there is sufficient linguistic evidence of its usage to include it as an individual dictionary entry, it shows that this particular meaning is univocal across speakers of the language and causes no problem of interpretation. The assumption of primary or literal meanings appears to play no important role here.

Even when we talk about disambiguating, we disambiguate between meanings that have equal status. Second, even if a meaning of a polysemous adjective has no dominant position in the language ('bright' in "bright sound"), there may be ways of fixing its referential status and giving it the flavour of objectivity which seems to be the most important reason for drawing the line between the literal and the metaphorical. Thus, should we ever need it

[106] There is, evidently, a qualitative shift from the ability to detect to the ability to represent, but it has no bearing on the present discussion.

(developing into a highly musical community, which we are not), criteria for the brightness of music[108] may be established just as well as we possess intersubjective criteria for the brightness of lights (contrary to those who believe that "bright" applies properly only to lights, brightness is not an objective property: physics deprecates it, preferring instead "luminance" – see Larousse Dictionary of Science and Technology). "Hot" is even more striking in this respect: criteria for the hotness of capsicum peppers are no less objective than criteria for the hotness of surfaces. What are the grounds for saying that "hot" as of temperatures is the literal meaning, while "hot" as of tastes is not for the present time slice of the English language? It looks as though there is none besides an *a priori* stipulation.

Another question concerning linguistic realization is that languages can have two or more terms corresponding to the same concept (being mapped onto the same psychologically primitive concept – recall Fodor's discussion of "addled"). Thus, Russian has two words – "ostryi" and "rezkyi" – which are translatable by the English "sharp". I would say that both "ostryi" and "rezkyi" map onto the same concept as the English "sharp" (call it 'S'), the only interesting thing to say about them is that "ostryi" is predicated of such and such objects and experiences while "rezkyi" is predicated of other objects and experiences. Moreover, the two are often interchangeable ("ostraya bol'" and "rezkaya bol'", which both translate as "sharp pain", are synonymous in Russian). Even when they are not used properly (to say "ostryi zvuk" instead of "rezkyi zvuk", translatable as "sharp sound"), interpretability is not barred. A similar example in the case of English may be found in the discussion of Marks et al.'s (1987) work (chapter 3), where the authors contrast "loud sounds" with "soft sounds" (whereas other native speakers would have preferred "quiet sounds").

There are other tricky points one may raise, and for which it is hard to find an answer. For instance, one may object to the no-polysemy view I am proposing here by saying that if brightness (in "bright sound") is an objective quality, it does not follow that the concept 'bright' which is being entertained in connection with "bright light" and "bright sound" is the same concept. As it stands, the objection is valid. However, there is presently no

---

[107] Interestingly, although Barsalou offers a perceptual theory of knowledge, one can do without the literal-metaphorical distinction in his framework, where concepts are taken to be the abilities to simulate a kind of thing perceptually (see his discussion of the concept 'leg' in section 4.1.2.)

[108] The following is a technical description from the Laney LC15 User Manual: "BRIGHT SWITCH: Adds brightness and sparkle to the upper frequencies of the amplifier." Clearly, it was not intended as a metaphorical instruction. Such examples are legion in musical and sound engineering literature: "There are a few slightly-varied substitutes for the 12AX7, such as the Russian 12AXR, which is somewhat *brighter*, with quicker compression and lower gain" (*Guitar Shop*, October 1996, p.84; my italics); "The perfectly tuned sound chamber and Duncan Seth Lover pickups offer up many of the inspiring tonal characteristics of a vintage 335 without the bulky body. Its clean tone is best described as *bright* and rich" (*Guitar Player*, October 1996, p.57, my italics).

alternative to my no-polysemy hypothesis. In all the theories of concepts I discussed in the previous chapter it is assumed that words are names for concepts, and that the identity of naming reflects the similarity in the conceptual structure. That seems to be a reasonable assumption. It would be preposterous to suppose that the concept 'good' is not univocal across fields but that there are as many concepts corresponding to the word "good" as there are fields in which "good" can be used. I take it that the same works for 'bright', 'sharp', etc.

Therefore, the real question here is whether a) it is the same concept that is being employed across fields (the view I am arguing for), whether b) there are metaphorical interconceptual connections in the conceptual structure (as suggested in experientialism), whether c) non-literal meanings of polysemous adjectives are not in the lexicon and their connection with the conceptual structure is mediated by extra-linguistic abilities (the position on which Pustejovsky and Fodor agree), or whether d) different literal meanings of polysemous adjectives correspond to different "entries" in the mental lexicon (the view Fodor sometimes endorses). I have considered all four of these stories. Experientialism runs into trouble with the idea of metaphorical structuring, and gives rise to two possible theories of concepts, neither of which is psychologically feasible (see Murphy 1996). Lexical semantics often does not claim to be a theory of concepts, and therefore it can put aside the issue of metaphorical polysemy. However, if a theory of meaning is to be a theory of what happens with the speakers of a language when they communicate in that language, it has to offer some connections with a theory of concepts. And that there are entities or processes inside the speakers' heads is shown by brain research (or at least, it is a useful assumption in cognitive psychology; apart from the references above; see Martin et al. 1995, Martin et al. 1996). Finally, the multiple-entry view is clear-cut and susceptible to scientific confirmation, but again it runs contrary to empirical evidence: psycholinguistic studies show that the meanings of polysemes are related in processing (and often it is a two-way influence: see the discussion of Williams 1992 above). Thus, those were also the reasons to prefer the no-polysemy view.

In many respects I subscribe to the Dretskian view of concepts: "without concepts we are blind to our intuitions" (1995: 135). We may experience properties, but it is only having concepts that can make us conscious of them. And the content of our experience is what it is since we evolved to be the kind of creatures that we are. Dretske's naturalism has it that what we find in our perceptual experience is the same as what we find to be real-world external properties; the way our experience represents objects to our consciousness is most likely the way things are. However, polysemy is not one of Dretske's concerns; that is why

there is no discussion of his work here. There is just a Dretskian story to tell which may help defend the present view. Concepts are products of a selectional process, hence there is animal continuity (i.e., we are not much different from animals with respect to how our central nervous system underlies our perceptual and categorizing abilities). Although animals may not have concepts – they cannot represent to themselves their experiences – they may have experiences similar to those that we designate "cold" experiences, i.e. where "cold" spans both cold surfaces and cold attitudes. Dretske assumes that animals are perceptually conscious (p. 121) of objects and their properties, which helps them discriminate, identify and recognize objects. It is plausible that the property 'cold' may be experienced in the animal world in its "many-sidedness": discriminating sexually willing from sexually unwilling members of the opposite sex requires that an animal's experience give rise to adequate beliefs (p. 119).

There may be simple associationist explanations for bringing close physical sensations and the experience of attitudes (such as those offered by Lakoff and Johnson 1980: ch.4). However, my claim was that with respect to the human conceptual system (in its present state) representational power must precede the ability to make associations in one's experience: before a field of experience may become structured on the model of another field, it has to possess its own internal structure; similarly, before a property may be compared with another property or labelled by association or analogy with that other property, there has to be a mind-world connection which establishes its existence in one's experience (this is another aspect demonstrated by Asch's and Marks et al.'s developmental studies; this is also the essence of my critique of experientialism). Still there is a possibility that the predecessor of the "unified" human conceptual system was the ability to make associations in the animal world. However, any question of this type requires scientific, not philosophical, answers.

The main purpose of my thesis is to show that it is possible to think of the conceptual system as containing concepts corresponding to polysemous adjectives, i.e. that all instances of "sharp" are mapped onto the concept 'sharp', all instances of "bright" are mapped onto the concept 'bright', etc. Empirical evidence suggests that this is possible. Therefore, when we talk about concept possession, we can say that to possess a concept 'bright', for instance, is to be able to respond to the world stimuli (linguistic and otherwise) in appropriate ways. In this sense, all humans (except for the severely impaired, like patients with dementia) possess the same concepts. Now the task is to connect this view of concepts with a plausible story about meanings. For it is clear that "bright" in "bright light" and "bright sound" cannot have the same meaning: in the first phrase it is predicated of light, in the second phrase it is

predicated of sound, lights and sounds are not the same, thus, their being bright cannot be the same property. This is how the standard reasoning goes and, supposedly, it is in contradiction with the no-polysemy view of conceptual structure proposed above. (Although such an argument has certain drawbacks itself: it would also require that "good" have different meanings in "good light" and "good sound". That, however, would be the wrong conclusion – see previous chapter).

This is how my story goes (the *a priori* assumption being that a theory of concepts does not necessarily have to be a theory of meaning at the same time). Psychologically primitive concepts are what polysemous adjectives are mapped onto, and what allows interpretations of those uses of an adjective that are novel for a speaker. To possess the concept 'bright' is to possess this psychologically primitive concept. However, to interpret concrete instances, such as "bright light" and "bright sound", something else is needed. It is to know, or to be able to extract from what one's experience represents, what it is for a light or for a sound to be bright. This is not achieved through processes of explicit reasoning (analogical reasoning), but through relevant exposure to the property in the real world. In this sense, a theory of meaning to satisfy the no-polysemy view would have to be externalist.

The schematic representation of the meaning of polysemous adjectives I proposed above was the following: bright/ of lights / having such and such properties/ testable by such and such procedures; bright/ of sounds/ having such and such properties/ testable by such and such procedures, etc. (where the slash marks stand for levels of representation, the left-most entry being a psychologically primitive concept). This may remind the reader of Pustejovsky's (1995) notion of co-composition. However, the difference is that in Pustejovsky's account meanings are constituted by introducing definitions into the structure of lexical items, while the present view requires only that meanings be constituted by mind-world relations. In this respect, it is similar to the much criticized view of enumerated meanings, which is largely accepted in philosophy: the meaning of "bright" in "bright light" and the meaning of "bright" in "bright sound" are constituted differently (the opposite view would be that the meaning of "bright" in "bright sound" is parasitic upon the meaning of "bright" in "bright light"; its extremes would be that "bright" in "bright sound" has only the speaker's meaning or no meaning at all, but the hearer's disposition to interpret it in relevant contexts).

Thus, within the no-polysemy view of conceptual structure we can retain the idea of meanings as denotations, but on condition that the way we establish meanings is not the way we establish concepts, and that the literal-metaphorical distinction is eliminated (at least with respect to synesthetic and double-function adjectives). According to the present view,

to know the meaning(s) of "bright" is to know what particular property it refers to when combined with different noun phrases. In this sense, pace Putnam 1975, experts are not needed to determine the meaning(s) of "bright": it is grounded in the primitive psychological concept "bright" which is the same for everyone. However, we may need to appeal to experts when/ if we need to identify the criteria of discrimination speakers of a language implicitly employ when categorizing stimuli into categories (e.g., brightness of light is determined in terms of luminance; to judge that a light is bright one does not even need to possess the concept 'luminance', but if we want to find out what lights speakers are likely to judge as bright, we need to employ the concept 'luminance', abstracted from individual experiences). Similarly, we may need to appeal to experts when/ if we need to establish what noun phrases "bright" may in principle combine with. However, since this cannot be done *a priori*, it is up to empirical studies to find out.

In sum, the evidence I discussed in the first four chapters is favourable towards the no-polysemy view I have spelled out in detail in this section. Researchers in empirical fields may agree with the views expressed here (Murphy (1996) and Miller and Johnson-Laird (1978) explicitly endorse the suggestion that there may be no relation of the polysemy/ metaphor type in conceptual structure, although they do not develop it any further). However, philosophers will be less likely to give up the literal-metaphorical distinction and their views on what criteria a theory of concepts and a theory of meaning have to satisfy. I will discuss some of the possible criticisms in the next section. However, since there may be no end to philosophical debates, the discussion will be restricted to those objections that appear to me to be the most interesting and challenging.

7. 3. Objections from philosophers.

On Fodor's account, any theory of concepts that reduces the majority of concepts to some primitives plus metaphorical or non-metaphorical extensions of those contributed by the semantic fields is doomed to fail. As he remarks, his critique of Jackendoff applies "without alteration" to cognitive linguistics claims (1998: 50). In my suggestions in the previous chapters I manage to avoid at least this criticism by stipulating that there are no "first" concepts (or meanings) that would be more primitive than others (hence, there are no definitions or extensions via definitions). Granted that I assume meanings to be non-inferential, it is still possible to wonder whether this option would not follow from the idea of psychologically primitive concepts. After all, if different instances of, say, "bright" are

mapped onto one and the same concept 'bright', it looks as though in all of the meanings of "bright" (bright/ of lights and bright/ of sounds) we discover the core, which is contributed by their being connected with the single concept 'bright'.

Although, indeed, I assume that the meanings of "bright" are connected, this does not imply that we derive the meaning of "bright" in "bright sound" by defining it in terms of the concept 'bright' plus a semantic field feature. The concept 'bright' is not definitional, it is an in-built device for detecting the property *brightness* in the real world. The problem, thus, consists in confusing two levels of description: concepts, which are mental entities, and meanings, which are public entities whose possession, however, is conditioned by our being the kind of beings that we are, and thus having the concepts that we have. The type of analysis I prefer is the one found in Fodor's analysis of "keep" discussed above. Similarly, Fodor notes that "exist" is not polysemous either between "chairs exist" and "numbers exist":

> "A familiar reply goes: the difference between the existence of chairs and the existence of numbers seems, on reflection, strikingly like the difference between numbers and chairs. Since you have the latter to explain the former, you don't also need 'exist' to be polysemic." (1998: 54)

This is precisely the idea that I have been trying to introduce with respect to what are ordinarily considered polysemous adjectives of the metaphorical type: to explain the difference between "bright light" and "bright sound" you do not need to have multiple entries in your mental lexicon because the difference is created by the difference between lights and sounds. You do not need either to introduce semantic information into your entries for 'light' and 'sound' because the interpretability of "bright light" and "bright sound" is achieved through the "accessibility" of denotations, the mind-world connections that lead, under normal conditions, to our responding to lights and to sounds in different ways. Similarly, for other polysemous adjectives I have discussed: "cold" does not have to be polysemous between "cold ice-cream", "cold light" and "cold person" because the difference is contributed by the difference between ice-creams, lights and persons, and we know that they are not the same, etc.[109] However, the price of the no-polysemy view I have proposed is parting with the literal-metaphorical distinction in conceptual structure, and as a result of it, in language for a large number of terms. And this option might be unacceptable to some. I contacted Fodor (e-mail communication, August 1999), trying to resolve the

[109] Clearly, a cold person need not be cold in all the senses of the word at the same time. I am presuming here that the default interpretation of "cold person" would be a psychological interpretation, since English has a different syntactic structure for the default physical interpretation with respect to the physical state ("He's cold" vs "He's a cold person").

inconsistency in the treatments of polysemy in Fodor (1998a) and Fodor and Lepore (1998). The question was what is to prevent us from treating the polysemy of taste predicates doubling as predicates for personality traits on the model of Fodor's (1998) analysis of "keep". Below I reproduce Fodor's objections to such a possibility and attempt to undermine them.

Essentially, all the objections are built around the notions of the literal and the metaphorical and the difference between the two. Thus, "sweet sugar" and "sweet girl" are different not only in that "sugar" and "girl" are different but in that "sweet sugar" is literal and "sweet girl" is not. Literalness becomes tied with a semblance of objectivity: when we say "sweet girl" we are praising the girl, but we are not commending a lump of sugar when calling it "sweet". Above I have doubted the presumption that "bright" is objective in "bright light" but not in "bright sound". The same considerations apply to the objectivity of "sweet" in "sweet sugar": what chemical analysis may establish is the presence or absence of compounds which have a certain effect on taste buds, but it cannot establish why their action on taste buds results in the 'sweet'-representation[110].

Second, metaphoricity becomes tied with evaluative judgments. But evaluation is not necessarily a part of statements involving taste predicates for describing personal characteristics: "He is a bitter person" may express a simple ascertaining of the fact which does not incorporate a personal judgment as to whether it is a good or a bad thing. Compare it with "He is tall": leaving aside its being a relative judgment, many people would agree that it contains "objective" information. However, the sentence itself may be used as an evaluative judgment (- "Why do you like him?" – "He's tall."). The same goes for "sweet". Whatever the chemical reasons, different brands of sugar may be of different (subjective) sweetness. Thus, in everyday speech we may find assertions of the type: "This sugar is too sweet" or "This sugar is not sweet enough" which combine an evaluative judgement with some objective statement (that sugar is sweet, not acid or bitter). Therefore, incorporation of an evaluative judgement cannot be a criterion for distinguishing literalness and metaphoricity.

Fodor's reasons for not accepting a single conceptual basis of adjectival polysemy do not go beyond the notion of literal truth. Thus, in his story "to keep the crowd happy" is literally true, while "to have a sweet temperament" is not. However, as discussed in the previous chapter, Fodor himself cannot avoid decomposing "keep" into 'keep having' and 'keep being', depending on the context in which "keep" is found. "To have a sweet temperament"

is not literally true only on the assumption that "sweet" has one meaning, which is related to taste sensations. However, the assumption bears an a priori character. If as a criterion of meaning we take the possibility of fixing the reference of an adjective in terms of intersubjective agreement, then we are fully entitled to individuate 'sweet/ of personalities', and the issue of the literal and the metaphorical is no longer relevant.

Approaches to semantics in philosophy, being synchronic, are often contrasted with diachronic linguistics. Interestingly, in Fodor's objections we may discern argumentation of the type that cognitive linguistics is fond of: the literal meaning is the historically first meaning. All other meanings of a term are derived via, say, Gricean mechanisms of interpretation. Again, apart from the untested assumption of a first meaning, there is nothing to support the claim about literalness. If 'sweet/ of personalities' is as much part of the language as 'sweet/ of tastes', it is not clear why a theory of language should not deal with it. If only the "first" meanings are considered literal, then only the physical meaning of "keep" should be considered literal. Fodor thus does not offer a unitary criterion of literalness[111].

At the beginning of chapter 4 I stipulated that a theory of meaning/ concepts, if it wants to satisfy the requirement of explanatory simplicity and coherence, should provide a single account for synesthetic and double-function meanings of a term. Thus, the way speakers produce and understand "cold person" should agree with the way they produce and understand "cold light" (that it indeed does so is shown by experiments with brain damaged populations discussed in chapter 5). In chapter 6 I showed that cognitive linguistics cannot provide such an account. According to Fodor's story, "sweet melody" cannot be literally true for the same reasons as "sweet temperament": "sweet" is only predicated of tastes, and melodies do not have a taste. Thus, we are back to the old argument of "proper" predication (see above).

However, if we can establish testable ways of fixing the reference of 'sweet/ of sounds', we shall be able to identify its meaning with denotation as objectively as we do for 'sweet/ of tastes'. And the fact that 'sweet/ of sounds' is not a matter of drawing analogies or

---

[110] If, à la Dretske, we hold that real-world properties are presented to us by our experience of them, it still has to be shown why the property 'sweet' (as in "sweet girl") cannot be a property delivered to us by our experience of it.

[111] The following is a quote from Fodor (1998b) which shows once again the dilemma that Fodor cannot solve with respect to polysemy: "Consider RED HAIR, which, I will suppose, is compositional (that is, not idiomatic) and applies to *hair that is red as far as hair goes*. The view of its semantics explains why, though red hair is arguably not literally red, still somebody who has RED and has HAIR and who understands the semantic implications of the syntactic structure AND, can figure out what "red hair" means. So, prima facie, RED HAIR is compositional and the demands of productivity are satisfied according to the present analysis." (p. 41). This is by and large the kind of analysis I subscribe to – take away the assumption of primary meanings.

applying Gricean considerations of relevance is reflected in the universal ability to form cross-modal associations. If, however, we do not consider adjectival polysemy to be part of the theory of language and assign it to the level of extralinguistic abilities (the likely candidates are higher level cognitive processes, such as analogical reasoning – see Fodor 1983), but still take thought to be computational and propositional, we will have to say from where "cold light" acquires its content. Since it has a fixable reference, it cannot be simply a matter of whatever the hearer may think in connection with it, and as such each of its constituents has to be mapped onto some entities in the mental lexicon. If 'cold/ of lights' is not mapped onto the primitive psychological concept 'cold', what is it mapped onto? There is not even a way to paraphrase "cold light" or to describe the lights that appear cold to us except for saying that they are... "cold".

The literal-metaphorical distinction may seem necessary to explain compositionality: in its strict sense the claim that meanings are context-invariant and that the meaning of the whole is determined by the meanings of its parts (see, for instance, Davidson 1968). The point of Fodor and Lepore's (1998) critique of Pustejovsky was preserving compositionality ("bake" cannot vary its meaning depending on the context because it either means 'bake1' or 'bake2' in all available contexts). How does the requirement of compositionality apply in the present case? If we want a language to be compositional, we have to concede that there is a number of distinct meanings for every term in it (since compositionality is compatible with ambiguity and meanings are context-invariant): for instance, "bright" must mean 'bright/ of lights', i.e. of a certain luminance. Since we cannot substitute "having luminance X" in the phrase "bright sound", "bright" in "bright sound" does not have the same meaning as "bright" in "bright light", and for that reason is not literal. The reply to such an objection is simple: you do not have to substitute "having luminance X" for any occurrence of "bright", because if you do, it would imply that the meaning of "bright" is not atomic or is not given by the mind-world connections but depends on the meaning of "luminance", etc., which is exactly the view that Fodor (1998a) proposed to avoid.

The no-polysemy view of concepts poses no problem of compositionality: we have as many meanings for "bright" as criteria for testing its applications by speakers of a language that we can devise. In "bright sound", "bright smell", "bright intelligence", etc. we are dealing with different meanings of the word "bright". However, in speakers' minds they are all mapped onto the same psychologically primitive concept 'bright'. And there is no contradiction here as long as we keep our theory of meanings and our theory of concepts separate. Concepts are in the head: you have the concept 'bright' and you have the concept 'sound', and knowing what it is for a sound to be bright in your world, you have the

compound concept 'bright sound' (the conjunction of the primitive concept 'bright', which corresponds to the multimodal perception of the property *bright*, and the concept 'sound'). Meanings are not in the head: the meaning of "bright" in "bright sound" is not the primitive concept 'bright' itself, but the set of auditory stimuli that will be judged bright by speakers of a language under favourable conditions. Whereas a theory of what it is for a sound to be bright should specify the set of conditions under which such judgement will be true with respect to the real world, stating those conditions in terms other than "brightness".

Nevertheless, the strongest philosophical objection comes perhaps from the view of metaphysical essentialism (of the Kripke (1972) type, see *Naming and Necessity*). Below is a straw story. I will not be naming names, but this kind of story may be told by anyone who holds that to determine the meaning of a statement we have to give its truth-conditions, where meanings of individual terms have to be given in terms of denotations, and those in terms of natural laws[112] (preferably, laws of physics). So, if we ask our straw philosopher why we use the same words to mark meanings (concepts) that are presumed unrelated (see Fodor and Lepore (1998) for instance: "meanings can overlap in all sorts of ways"), the answer will be that such words (e.g., "hot" in temperature and taste readings) are ambiguous, and that diachronical considerations are irrelevant to synchronic semantics. If we further ask why we also use "hot" to mean that someone is emotionally aroused, etc. (the general question of double-function adjectives), the answer will be that metaphor is irrelevant to semantics. And it is a metaphor because "hot" does not really have this meaning. It is only a matter of what speakers mean to say, and whether their hearers understand them in the intended way, the relationship between the speaker's and the hearer's understanding of the intended meaning being unreliable.

Now, we are going a little deeper. "Hot" (of temperature) refers to certain kinetic energy of molecular motion (we have discovered that increasing heat corresponds to increasing molecular motion – Kripke 1972: 129). Therefore, we can establish the truth-conditions of "This is hot" (ignoring the problem of indexicals for simplicity of exposition) if we have a mark on our scale which tells us that everything above it qualifies as hot. "Hot" (of taste, since we have admitted that "hot" is ambiguous between the two readings) refers, presumably, to the presence of vanilloid contents. Therefore, we can establish the truth-conditions of "This is hot" if we have a mark on our scale telling us how much vanilloid content is "hot". "Hot" (of temper) does not refer to anything in itself, as it were, since in this case we are talking about interpretation, not meaning. If it happens that we need to

[112] An elaboration: not of all terms in general, but natural kind terms. However, the adjectives in question may be considered natural kind terms.

establish the truth-conditions of "He's got a hot temper", we translate "hot temper" into, say, "violent temper" and then do the checking. The following argument is also likely to be offered: speakers and hearers may well understand the phrase "hot temper", but whatever the basis for their finding similarity between a violent temper and the rate of movement of molecules it is not a matter of semantics.

The argument seems incontestable at first. However, I am proposing to deal here not with a theory of meaning, but with a theory of concepts and conceptual structure. Unless we take concepts to be abstract Fregean entities (for why we may not want to do so see Fodor 1998a: 15-22), we take them to be things "in the head" (things that can be lost following brain damage). In this respect, I find that it is not accidental that speakers use the same words for designating different sensations, and why they should do so is definitely part of a theory of concepts (see previous section). Apart from that, there are other considerations that make the above argument sound doubtful. First of all, there is the problem of objectivity. It is stipulated that the meaning of "hot" (temperature) is constituted by the speakers use of language (whenever they are prone to call something "hot") and real-world facts (heat is kinetic energy of molecular movement). It should follow that only a certain rate of molecular motion can give a reliable connection with the speakers' use of "hot", and this will be the meaning of "hot". But consider. Sticking mustard plasters, once popular in treating chest colds, reliably produce the sensation of hotness among statistically large populations (the same as touching hot burners – the threshold of heat sensitivity may differ among individuals). However, there is no change in kinetic energy when a plaster comes into contact with human skin[113]. Is the resulting sensation of hotness an illusion?

Similarly with "hot" (to taste). Indeed the presence of vanilloid content correlates reliably with subjective ratings of the hotness of peppers (for more details on what is being discussed in this paragraph see chapter 2). Also, we separate the two readings of "hot" into two meanings because there is no correlation between the two real-world properties in terms of kinetic energy[114]. But on the other hand, the VR1 channel serves as the mechanism of transduction for both noxious temperature and capsaicin binding (the "hot" taste sensations). Since, objectively, noxious temperature uses the same elements as vanilloids, the resulting sensation may well be the same sensation. However, the speakers of English are able to distinguish between the two: "This soup is hot" is ambiguous between "The soup has been heated up to a high temperature" and "The soup is spicy". Thus, making meanings a matter

[113] Nor are there any chemical reactions apart from those that take place inside the body (the nerve endings).

[114] See Clapham (1997): "It is possible that the real purpose of the channel [VR1] is to sense noxious temperature, and that capsaicin is nature's low energy way of harnessing the sensor."

of real-world properties independent of the perceiver cannot explain why they are connected in the perceiver's mind even in the presence of evidence that they are connected in the real world, and metaphysical essentialism cannot serve as a basis for a theory of concepts[115].

Finally, about double-function adjectives. The reason impelling one to say that "hot" does not have a psychological meaning (but an interpretation) is that we do not have any physical theory for what it is for a temperament to be hot. The discovery of the VR1 (or the hot channel) is quite a recent one, and it allowed us to say that "hot" is ambiguous between "hot" (of temperature) and "hot" (to taste). Presumably, before the discovery was made, "hot" had to be considered as having only one literal meaning, and "hot" (to taste) was not a matter of meaning but of interpretation, which was parasitic upon "hot" (of temperature). But has anything changed in the way speakers use "hot" for designating their taste sensations in the mid-90s? Apparently, the majority of them have not even heard of the discovery. There are arguments in philosophy to the effect that speakers may be mistaken in their usage of terms (Kripke 1972, Putnam 1975), but then we may come to the view that the speakers' use of language does not contribute to determining meanings as much as facts about the real world.

Consider. In the case of "hot" (to taste) we had a usage which later received scientific explanation/ justification. In the case of "hot" (of temper) we have a usage for which we do not have scientific confirmation. There are two options open to us: either we assume that "hot" (of temper) only presently has the status of interpretation, but may receive the status of meaning should we manage to reduce the psychological to the physical, or we assume that "hot" (of temper) cannot ever be a matter of meaning since the reduction is not feasible. Choosing the second option, we come very close to admitting that meanings (or rather, literal meanings) are in a sense independent of the speakers' use of language (since we have no scientific grounds which would allow us to say when the speakers are mistaken and when they are not), and this does not help us much with clarifying what concepts are or might be[116]. Choosing the first option, we admit that the literal-metaphorical distinction is only

---

[115] The no-polysemy view avoids this problem as the difference between *hotness* (temperature) and *spiciness* is placed not into the real-world properties but into speakers' physiological responsiveness: although the mechanisms of transduction for noxious heat and vanilloid content largely overlap, the information that reaches the cortex and becomes translated into a representation of subjective experience is both the information about the quality of received stimulation decomposable into particular sensory characteristics (hotness and spiciness) and the information about the similarity of sensory experiences (spiciness as hotness).

[116] There are other problems with this option. Thus, assuming that "hot" (of temper) has no meaning we are facing the trouble of explaining the cross-linguistic evidence which shows that in unrelated languages and cultures "hot" receives the same interpretation. Cf. Brown (1958): "No case was discovered in which the morpheme for *hot* named a remote, calm (in fact, *cool*) manner" (p. 146). Also, if we say that in order to test the truth-conditions of "He has a hot temper" we have to translate

valid retrospectively, for unconfirmed hypotheses. But this is in no contradiction with the view of psychologically primitive concepts I proposed above since my major claim is that there cannot be a literal-metaphorical distinction within the conceptual system itself. I deny the literal-metaphorical distinction in language only insofar as language is related to the conceptual system. I admit however that at a given point in time certain meanings can have a status either of literal or figurative meanings depending on the degree of their conventionality in the linguistic community and the availability of means for testing their reference.

The issue of polysemy (metaphorical polysemy) is presumably of some interest to philosophy (Fodor's attacks on anyone wishing to introduce discussions of polysemy into semantics show this). One may choose to ignore it, moving the discussion onto the level of truth-conditions, but the way speakers use their language (and the amount of evidence now gathered thanks to cognitive linguistics' high popularity) suggests that polysemy, and relations between different meanings of polysemous terms, are not accidental to language. A philosophical attitude reflecting such awareness is found in Blackburn (1984; since it is a good quote, it is going to be a long quote):

> "Consider the way in which we use terms which also describe physical affairs to describe psychological affairs. We think of matches in boxes, and thoughts in the head; of being pulled by ropes, or being pulled by desires; of jumping to attention, or jumping to a conclusion. Metaphorical, of course. Or is it? We most probably cannot cash the metaphors, either by giving a single literal way of saying something which they yield or even by indicating a range of comparisons which they suggest. On the other hand we should not be happy with just postulating two different senses of the terms: it is not pure accident that we talk of thoughts in the heads, for instance. If it were simply a case of the word 'in' being ambiguous we might just as well have planted the ambiguity on some other term, and talked of terms being *on* the head, for instance. Yet we should also be unhappy with the idea that there is a smooth or natural extension of the terms from the physical to the mental. Someone who understands what it is for one thing to be in another physically has a definite range of capacities: he can find things, obey instructions, perform a range of tasks which exhibit or exercise this understanding. These tasks are not the same as any (whichever they might be!) which exercise an understanding of thoughts being in the head." (p. 178)

To summarize. The philosophical problem that polysemous terms cause consists in two irreconcilable observations. One is that the testing procedures for different meanings of a polysemous term do not overlap in any interesting way and this is obvious not only to experts, but to the speakers of a language as well. The second is that many cases of

---

"hot" into "violent", then we presuppose that we have a scale for violence (a behavioural scale?), but it is not clear that we have. I have to stress, though, that the second option is not the one that Kripke and Putnam themselves would prefer (but other philosophers might – namely, those who hold that the

polysemy cannot be accidental: we have cross-linguistic, psycholinguistic and clinical psychological evidence that they are not. Since the two observations appear irreconcilable, there are different traditions that study how languages work and how they relate to human cognition. This thesis was an attempt to reconcile on a small scale (with respect to polysemous adjectives only) the idea that different meanings of a polysemous term are related (the tradition of cognitive linguistics), the idea that meanings are grounded in perception (the tradition of contemporary psychological research), the idea that meanings can be produced combinatorily (the tradition of lexical semantics), and the idea that meanings are denotations (the tradition of truth-conditional semantics). I have attempted to show that the proposed no-polysemy view, which assumes the existence of psychologically primitive concepts spanning the whole variety of domains of human experience is sound both empirically and philosophically. Whether this hypothesis can be confirmed will depend on future research into the issues raised here.

---

reduction of the psychological to the physical is impossible in principle). If I read Putnam and Kripke correctly, they both allow that even if "hot temper" is not yet literal it can become so.

Conclusions and implications.

This thesis presents a new outlook on the issue of metaphorical polysemy offering an extensive study of synesthetic and double-function adjectives. Combining discussions of empirical evidence from a variety of disciplines with a philosophical analysis it proposes a new framework for approaching concepts and the conceptual structure. The ideas from linguistics, psychology and philosophy drawn upon are not new. However, the way they are put together is likely to incur critique from scholars in different traditions of the aforementioned fields of inquiry. Interdisciplinary studies are particularly attractive as they are the best candidates for providing new insights into the nature of our quests, and it is quite difficult to understand the antagonism with which philosophers often meet theories in linguistics and psychology and with which linguists and psychologists often speak of philosophical ideas. This thesis is an attempt, for a narrowly defined topic, to fuse my fondness and respect for philosophy with evidence collected by cognitive linguistics and other empirically oriented studies. For the reasons of its novelty and, in many respects, eccentricity, it could not but be programmatic. Some doubts may have been resolved but much remains to be clarified and refined, both in its empirical and theoretical aspects. Nevertheless, this thesis can serve as a good start for redirecting our inquiries so that the best ideas in disciplines dealing with human language and cognition would work for their mutual benefit. Below I summarize the hypothesis proposed and outline the directions which further inquiry may take.

Assuming that the understanding and processing of synesthetic and double-function adjectives should be subsumed by the same mechanisms, on the basis of data discussed in chapters 2-5, a view labelled the no-polysemy view of concepts was put forward. This view holds that there are psychologically primitive concepts which are mental "units" allowing us to detect the same property in those modalities and experiences through which we gather information about the external world. Concepts are thus mind-world relations, ways of categorizing stimuli. Another assumption made and argued for here is that concepts are innate and atomic. During processing, a polysemous adjective is mapped onto the corresponding concept, and its meaning is computed on the basis of combinatorial principles, which take into account the property that the adjective corresponding to the concept is tuned to and the possible manifestation of that property in the denotation of the noun phrase with which the adjective in question is being combined (think of the "bright" example). Thus, polysemous adjectives in all their meanings are mapped onto one and the same concept. Conceptual structure of this type is necessarily grounded in perception and

may share common neural systems with perceptual mechanisms, but the psychologically primitive concepts cannot have ontogenetically empirical origin. They are representational primitives (cf. Pylyshyn 1999: "A concept is supposed to be not just a form of physiological reaction, but an intentional, mind-bearing entity with a role to play in reasoning and inference"). This way of construing concepts corresponding to polysemous adjectives makes the literal-metaphorical distinction in this case an artificial one. Concepts exhibit no metaphoricity; metaphoricity (at least with respect to the adjectives in question) that seems to be found in languages is the reflection of linguistic conventionality and our theories of language, not of properties of the conceptual structure. Finally, within the framework proposed we can preserve the idea of meanings as denotations under a loose understanding of compositionality. Meanings are not concepts, but the knowledge of meanings of polysemous adjectives depends reliably on the possession of corresponding concepts.

What about implications? I believe that with respect to metaphor not enough discriminations have been made. Cognitive linguistics opened up a whole new field of enquiry suggesting that the majority of concepts are metaphorically derived. It is clear now that for synchronic semantics or for a theory of concepts this is only a useful exaggeration calling attention to an understudied issue. I suggest that on a conceptual perspective (rather than a linguistic perspective), metaphors and non-metaphors have to be discriminated by what is part of conceptual structure and what is not. I showed that for polysemous adjectives of synesthetic and double-function types we can do without the notion of metaphor. The same may apply to other grammatical categories: verbs (like "rise", see Murphy 1996:190), nouns (like "slice": it is dubious that "time slice" is a derivative of "slice of bread" since there are no grounds for comparison that would justify a metaphorical transfer), prepositions (like "in", see Jackendoff 1998: 206). The issue of logical form (or syntactic properties of expressions) which I avoided in my exposition, but which evidently bears on the question of interpretation and processing, requires more study (see Hanks 1999 for his analysis of literalness vs. metaphoricity with respect to nominal constructions). Perhaps we shall see that unextended combinations favour the "literal" reading (as in "He is cold") and that "metaphorical" readings tend to occur in certain syntactic patterns (as in "He is a cold person"). Nonetheless, this will not free us from the need to explain the conceptual correlates of polysemous terms.

In those cases where it is apparent that the seeming metaphoricity of an expression cannot be explained by the existence of an innate psychologically primitive concept, we are most probably dealing with true instances of metaphor. Again, we will have to give an account of how their production and understanding are made possible; we will have to

postulate or appeal to cognitive mechanisms that underlie it. My guess is that we do not need an independent mechanism of metaphorical projection, and that all such instances can be subsumed by the mechanisms of analogical reasoning as part of inductive cognitive processes (see Sternberg 1982). The issue of analogical reasoning as underlying metaphor formation is an exciting one, but the considerations of space preclude a discussion of it here. There is a vast amount of literature on analogical reasoning and I would agree with many ideas found there (see, for instance, Miller 1979, Gick and Holyoak 1980, Holyoak 1982, Holyoak and Thagard 1989, Dejong 1989, Gentner 1989, Bowdle and Gentner 1997, Gentner and Wolff 1997).

Further research which may confirm or disconfirm the proposed hypothesis will have to cover a large number of fields. They will have to include cross-linguistic and cross-cultural studies (although one can accommodate and re-analyse data gathered in the cognitive linguistics tradition) in order to see how far the overlaps in polysemous terms extend. It should be possible to give an explanation for the preferential character of some mappings compared to others (see Williams 1975), although we shall no longer be talking of "mappings", but of property distribution (what things in the real world may appear to us as possessing a certain property). Pro- or counterevidence may come from psycholinguistic studies, where it could be tested more extensively than in Williams (1992) how the different meanings of polysemes are related during on-line processing. Some decisive evidence may be acquired from neurolinguistic studies, shedding light on whether we could distinguish neural configurations corresponding to what I called psychologically primitive concepts (see Young 1989 for encouraging views on concept innateness and atomism). Research in developmental psychology may provide contrasting evidence for Asch's study, showing whether and how different meanings of polysemes are related in concept acquisition. Research in animal reasoning may help us see the ways in which we could have evolved into beings who possess languages crammed with polysemy. There is evidence that higher primates are capable of cross-modal transfers (see, for instance, Elman et al. 1989, chapter 5), and perhaps this is where we should look more closely.

Whatever evidence we discover, we also have to interpret it coherently. In my thesis I showed a way to interpret the evidence concerning synesthetic and double-function adjectives. It may not be the right way, but it is at least a viable alternative to other possible interpretations. There are other cases known when the same evidence has been interpreted in a diametrically opposite way (the best to my knowledge is Fodor 1983 and the discussion it provoked in Fodor 1985). There are also cases known when the interpretation proposed by the researchers themselves could not be considered workable (see, for instance, discussions

in Caplan 1987, chapter *Disturbances of lexical semantic representations*). Whatever evidence we discover, the explanations we provide have to be philosophically coherent, and it would be preposterous to ignore the wealth of argumentation developed in philosophical semantics. But it is not fruitful either to ignore the evidence we obtain from empirical studies. The best way is in the middle, in finding ways to bring evidence and philosophical coherence as close as possible. This thesis was an attempt to do exactly this for the issue of polysemous adjectives and conceptual structure.

Bibliography

Adair, J. C., R. L. Schwartz, D. L. Na, E. Fennell, R. L. Gilmore, and K. M. Heilman (1997), Anosognosia: examining the disconnection hypothesis. *Journal of Neurology, Neurosurgery and Psychiatry*, **63**, 798-800.

Asch, S. E. (1955), On the use of metaphor in the description of persons. In H. Werner (Ed.), *On Expressive Language*. Clark University Press.

Asch, S. E. (1958), The metaphor: a psychological inquiry. In R. Tagiuri and L. Petrullo (Eds.), *Person Perception and Interpersonal Behavior*. Stanford, CA: Stanford University Press.

Asch, S. E. and H. Nerlove (1960), The development of double function terms in children. In B. Kaplan and S. Wapner (Eds.), *Perspectives in Psychological Theory*. New York: International Universities Press, Inc.

Badecker, B. and A. Caramazza (1985), On considerations of method and theory governing the use of clinical categories in neurolinguistics and cognitive neuropsychology: the case against agrammatism. *Cognition*, **20**, 97-125.

Badeer, H. S., and J. W. Hicks (1992), Hemodynamics of vascular waterfall - is the analogy justified? *Respiration Physiology*, **87**, 205-217.

Barsalou, L. W. (1999), Perceptual symbol systems. *Behavioral and Brain Sciences*, **22**, 577-633.

Békésy, G. von (1959), Similarities between hearing and skin sensations. *Psychological Review*, **66**, 1-22.

Billow, R. M. (1977), Metaphor. A review of the psychological literature. *Psychological Bulletin*, **84**, 81-92.

Blackburn, S. (1984), *Spreading the Word: Groundings in the Philosophy of Language*. Oxford: Clarendon Press.

Bloom, P. (2000), *How Children Learn the Meanings of Words*. Cambridge, MA: MIT Press.

Bowdle, B. F. and D. Gentner (1997), Informativity and asymmetry in comparisons. *Cognitive Psychology*, **34**, 244-286.

Braak, H., U. Rub, D. Sandmann-Keil, W. P. Gai, R. A. I. de Vos, E. N. H. J. Steur, K. Arai and E. Braak (2000), Parkinson's decease: affection of brain stem nuclei controlling premotor and motor neurons of the somatomotor system. *Acta Neuropathologica*, **99**, 489-495.

Bris, S, C. L. Gerard and J. L. Adrien (1999), Development of "theory of mind" and Piagetian conservation stage with dysphasic children. *ANAE - Approche Neuropsychologique des Apprentissages chez L'enfant*, **11**, 49-58

Brown, J. W. (1997), Process and creation. In A. Andersson and N.-E. Sahin (Eds.), *The Complexity of Creativity* (pp. 35-50), Dordrecht: Kluwer.

Brown, R. (1958), *Words and Things*. The Free Press of Glencoe.

Brownell, H. H., H. H. Potter, D. Michelow and H. Gardner (1984), Sensitivity to lexical denotation and connotation in brain damaged patients: A double dissociation? *Brain and Language*,**100**, 717-729.

Brownell, H. H., T. L. Simpson, A. M. Bihrle, H. H. Potter and H. Gardner (1990), Appreciation of metaphoric alternative word meanings by left and right brain damaged patients. *Neuropsychologia*, **28**, 375-383.

Buhmann, J. M., J. Malik, P. Perona (1999) Image recognition: Visual grouping, recognition, and learning. *Proceedings of the National Academy of Sciences of the United States of America*, **96**, 14203-14204.

Bush, G., P. Luu and M. I. Posner (2000), Cognitive and emotional influences in anterior cingulate cortex. *Trends in Cognitive Sciences*, **4**, 215-222.

Caplan, D. (1987), *Neurolinguistics and Linguistic Aphasiology*. Cambridge: Cambridge University Press.

Carey, S. (1982), Semantic development: the state of the art. In L. Gleitman and E. Wanner (Eds.), *Language Acquisition: The State of the Art*. New York: Cambridge University Press.

Carlson, R. A. (1997), Meshing Glenberg with Piaget, Gibson, and the ecological self. *Behavioral and Brain Sciences*, **20**, 21 et seq.

Carterette, E. C. and Friedman, M. P. (Eds.) (1973), *Handbook of Perception*, volume III *Biology of Perceptual Systems*. Academic Press, New York and London.

Cassell's English-Dutch, Dutch-English Dictionary (1980), completely revised by J. A. Jockin-La Bastide, G. van Kooten and J. Kramers. London: Cassel.

Caterina, M. J., M. A. Schumacher, M. Tominaga, T. A. Rosen, J. D. Levine, and D. Julius (1997), The capsaicin receptor: a heat-activated ion channel in the pain pathway. *Nature*, **389**, 816-824.

Caterina, M. J., T. A. Rosen, M. Tominaga, A. J. Brake and D. Julius (1999), A capsaicin-receptor homologue with a high threshold for noxious heat. *Nature*, **398**, 436-441.

Caterina, M. J., A. Leffler, A. B. Malmberg, W. J. Martin, J. Trafton, K. R. Petersen-Zeitz, M. Koltzenburg, A. I. Basbaum and D. Julius (2000), Impaired nociception and pain sensation in mice lacking the capsaicin receptor. *Science*, **288**, 306-313.

Chiarello, C. (1988), Semantic priming in the intact brain: Separate roles for the right and the left hemispheres? In Chiarello, C. (ed.), *Right Hemisphere Contributions to Lexical Semantics*. New-York: Springer-Verlag.

Chobor, K. L. and A. Schweiger (1998), Processing of lexical ambiguity in patients with traumatic brain injury. *Journal of Neurolinguistics*, 11, 119-136.

Chomsky, N. (1980), *Rules and Representations*. Oxford: Basil Blackwell.

Chomsky, N. (1986), *Knowledge of Language: Its Nature, Origin, and Use*. Praeger.

Chomsky, N. and Lasnik, H. (1993), The theory of principles and parameters. In J. Jacobs, A. von Stechow, W. Sternefeld and T. Vennemann, *Syntax: an International Handbook of Contemporary Research*, 506-569. Berlin: Walter de Gruyter.

Churchland, P. M. (1991), *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science*. Cambridge, MA: MIT Press.

Clancy, B., R. B. Darlington, B. L. Finlay (2000), The course of human events: predicting the timing of primate neural development. *Developmental Science*, **3**, 57-66.

Clapham, D. E. (1997), Some like it hot: spicing up ion channels. *Nature*, **389**, 783-784.

Clark, M. A., A. S. Merians, A. Kotkari, H. Poizner, B. Macauley, L. J. G. Rothi, and K. M. Heilman (1994), Spatial planning deficits in limb apraxia. *Brain*, **117**, 1093-1106.

Claudi, U. and B. Heine (1986), On the metaphorical basis of grammar. *Studies in Language*, **10**, 297-335.

Collins Concise Spanish-English English-Spanish Dictionary (1985), edited by M. Gonzalez. London: Collins.

Collins German-English English-German Dictionary (1993), edited by P. Terrell. Glasgow: HarperCollins.

Collins and Robert French-English English-French Dictionary (1987), edited by B. T. Atkins. Glasgow: HarperCollins, Paris: Le Robert.

Curtis, M. (1985), Children's understanding of polysemous terms. Abstract of the talk presented at Developmental Psychology Section Conference, 14-17 September 1984, Lancaster University. *Bulletin of the British Psychological Society*, **38**, A17.

Cytowic, R. E. (1989a), *Synesthesia: A Union of the Senses*. Springer-Verlag New York Inc.

Cytowic, R. E. (1989b), Synesthesia and mapping of subjective sensory dimensions. *Neurology*, **39**, 849-850.

Dahl, O. (1989), Women, Fire and Dangerous Things: What Categories Reveal about the Mind - Lakoff, G. (Book Review). *Linguistics*, **21**, 1143-1152.

Damasio, H. and A. R. Damasio (1980), The anatomical basis of conduction aphasia. *Brain*, **103**, 337-350.

Davidson, D. (1978), What metaphors mean. In: S. Sacks (Ed.), *On Metaphor*. Chicago: The University of Chicago Press, pp.29-45.

Davis, J. B., J. Gray, M. J. Gunthorpe, J. P. Hatcher, P. T. Davey, P. Overend, M. H. Harries, J. Latcham, C. Clapham, K. Atkison, S. A. Hughes, K. Rance, E. Grau, A. J. Harper, P. L. Pugh, D. C. Rogers, S. Bingham, A. Randall and S. A. Sheardown (2000), Vanilloid receptor-1 is essential for inflammatory thermal hyperalgesia. *Nature*, **405**, 183-187.

Dawkins, R. (1976), *The Selfish Gene*. Oxford: Oxford University Press.

Dejong, G. (1989), The role of explanation in analogy; or, the curse of an alluring name. In S. Vosniadou and A. Ortony (Eds.), pp. 346-365.

Dennett, D. C. (1995), *Darwin's Dangerous Idea*. New York: Simon and Schuster.

Dretske, F. (1969), *Seeing and Knowing*. Chicago: University of Chicago Press.

Dretske, F. (1995), *Naturalizing the Mind*. Cambridge, MA: The MIT Press.

Dummett, M. (1973), *Frege: Philosophy of Language*. New York: Harper & Row.

Durkin, K. and J. Manning (1989), Polysemy and the subjective lexicon: Semantic relatedness and the salience of intraword senses. *Journal of Psycholinguistic Research*, **18**, 577-612.

Dusek, J. A. and H. Eichenbaum (1997) The hippocampus and memory for orderly stimulus relations. *Proceedings of the National Academy of Sciences of the United States of America*, **94**, 7109-7114.

Edelman, G. M. (1992), *Bright Air, Brilliant Fire: On the Matter of Mind.* Penguin Books.

Elman, J. L., E. A. Bates, M. H. Johnson, A. Karmiloff-Smith, D. Parisi and K. Plunkett (1996), *Rethinking Innateness: A Connectionist Perspective on Development.* Cambridge, MA: MIT Press.

English-Greek Dictionary: a vocabulary of the Attic language (1932), edited by S. C. Woodhouse. London: G. Routledge and sons.

Ettlinger, G. (1960), Cross-modal transfer of training in monkeys. *Behavior*, 16, 56-65.

Fernandez, J. W. (Ed.) (1991), *Beyond Metaphor: The Theory of Tropes in Anthropology*. Stanford University Press, Stanford, California.

Fields, H. L. (1987), *Pain*. McGraw-Hill Book Company, USA.

Fodor, J. A. (1970), Three reasons for not deriving "kill" from "cause to die". *Linguistic Inquiry*, **1**, 429-38.

Fodor, J. A. (1975), *The Language of Thought*. New York: Crowell.

Fodor, J. A. (1980), Fixation of belief and concept acquisition (with the subsequent discussion). In M. Piatelli-Palmarini (Ed.), pp. 142-162.

Fodor, J. A. (1983), *The Modularity of Mind: An Essay on Faculty Psychology*. Cambridge, MA: MIT Press.

Fodor, J. A. (1987), *Psychosemantics: The Problem of Meaning in the Philosophy of Mind.* Cambridge, MA: MIT Press.

Fodor, J. A. (1997), Do we have it in us? Review of J. Elman et al. *Rethinking Innateness. The Times Literary Supplement*, May 16.

Fodor, J. A. (1998a), *Concepts: Where Cognitive Science Went Wrong*. Oxford: Clarendon Press.

Fodor, J. A. (1998b), There are no recognitional concepts – Not even RED. In his *In Critical Condition: Polemical Essays on Cognitive Science and the Philosophy of Mind.* Cambridge, MA: MIT Press, 35-47.

Fodor, J. A. (1998c), Review of Steven Mithen's *The Prehistory of the Mind.* In his *In Critical Condition: Polemical Essays on Cognitive Science and the Philosophy of Mind.* Cambridge, MA: MIT Press, 153-160.

Fodor, J. A. and E. Lepore (1992), *Holism: A Shopper's Guide*. Oxford: Blackwell.

Fodor, J. A. and E. Lepore (1998), The emptiness of the lexicon: Reflections on James Pustejovsky's *The Generative Lexicon. Linguistic Inquiry*, **29**, 269-88.

Freides, D. (1974), Human information processing and sensory modality: cross-modal functions, information complexity, and deficit. *Psychological Bulletin*, **81**, 284-310.

Gainotti, G., M. C. Silveri, A. Daniele and L. Giustolisi (1995). Neuroanatomical correlates of category-specific semantic disorders: A critical survey. *Memory,* **3**, 247-264.

Gardner, H. (1974), Metaphors and modalities: How children project polar adjectives onto diverse domains. *Child Development*, **45**, 84-91.

Garnham, A. (1989), Women, Fire and Dangerous Things: What Categories Reveal about the Mind - Lakoff, G. (Book Review). *Quartile Journal of Experimental Psychology*, **41**, 415-417.

Garrett, M. F. (1976), Syntactic processes in sentence production. In R. Wales and E. Walker (Eds.), *New Approaches to Language Mechanisms*. North-Holland, Amsterdam.

Garrett, M. F. (1984), The organization of processing structure for language production: applications to aphasic speech. In D. Caplan, A. R. Lecours and A. Smith (Eds.), *Biological Perspectives on Language*. Cambridge, MA: MIT Press

Gauthier, I. and N. K. Logothetis (2000), Is face recognition not so unique after all? *Cognitive Neuropsychology*, **17**, 125-142.

Geach, P. (1957), *Mental acts: Their content and their objects*. London: Routledge and Kegan Paul.

Geert, P. van (1998), A dynamic systems model of basic developmental mechanisms: Piaget, Vygotsky, and beyond. *Psychological Review*, 105, 634-677.

Gentner, D. (1983), Structure-mapping: a theoretical framework for analogy. *Cognitive Science*, **7**, 155-170.

Gentner, D. (1988), Metaphor as structure-mapping: the relational shift. *Child Development*, **59**, 47-59.

Gentner, D. (1989), The mechanisms of analogical learning. In S. Vosniadou and A. Ortony (Eds.), pp.199-241.

Gentner, D. and Stuart, P. (1983), *Metaphor as Structure-Mapping: What Develops.* (Tech. Rep. 5479). Cambridge, MA: Bolt, Beranek & Newman.

Gentner, D. and Toupin, C. (1986), Systematicity and surface similarity in the development of analogy. *Cognitive Science*, **10**, 277-300.

Gentner, D., B. Falkenhainer and J. Skorstad (1988), Viewing metaphor as analogy. In D. H. Helman (Ed.), pp.171-177.

Gentner, D. and C. A. Clement (1989), Evidence for relational selectivity in the interpretation of analogy and metaphor. In G. H. Bower (Ed.), *The Psychology of Learning and Motivation.* New York: Academic Press.

Gentner, D. and P. Wolff (1997), Alignment in the processing of metaphor. *Journal of Memory and Language*, **37**, 331-355.

Georgopoulos, A. P. (1974), Functional properties of primary afferent units probably related to pain mechanisms in primate glabrous skin. *Journal of Neurophysiology*, **39**, 71-83.

Geschwind, N. (1964). In C. J .J. M. Stuart (Ed.), *Monograph Series on Language and Linguistics, No 17* (p.155). Washington, D.C.: Georgetown University.

Geschwind, N. (1965), Disconnection syndromes in animals and man. *Brain*, 88, 237-294 and 585-644.

Gibbs, R. W., Jr. (1992), When is metaphor. *Poetics Today*, **13**, 575-606.

Gibbs, R.W., Jr. (1996), Why many concepts are metaphorical. *Cognition*, **61**, 309-319.

Gick, M. L., and Holyoak, K.J. (1980), Analogical problem solving. *Cognitive Psychology*, **12**, 306-355.

Glenberg, A. M. (1997), What memory is for. *Behavioral and Brain Sciences*, **20**, 1-19.

Goatly, A. (1997), *The Language of Metaphors.* London: Routledge.

Goddard, C. (1999), A cross-linguistic and cross-cultural perspective on "active" metaphors. Keynote lecture 2 at RAAMIII conference, 30 June –2 July 1999, Tilburg, the Netherlands.

Goldstein, K. (1948), *Language and Language Disturbances.* New York: Grune and Stratton.

Goswami, Usha (1992), *Analogical Reasoning in Children.* Lawrence Erlbaum Associates.

Grady, J., S. Taub and P. Morgan (1996), Primitive and compound metaphors. In A. Goldberg (Ed.), *Conceptual Structure, Discourse and Language.* Stanford: CSLI/ Cambridge.

Greek-English Lexicon (1966/ 1845), compiled by H. G. Liddell and R. Scott. Oxford: Clarendon Press.

Hanks, P. (1999), How to tell a meaning from a metaphor. Paper presented at AISB'99 Workshop on Metaphor, Artificial Intelligence and Cognition, Edinburgh, 4 -6 June.

Harnad, S. (2000) Correlation vs. causality: How/ why the mind/ body problem is hard. *Journal of Consciousness Studies*, **7**, 54-61.

Harrap's New Standard French and English Dictionary (1972-1980), edited by J. E. Mansion. London: Harrap.

Harrap's Standard French and English Dictionary (1980), edited by J. E. Mansion, revised and edited by D. M. Ledesert and R. P. L. Ledesert. London: Harrap.

Hart, F. E. (1995), Cognitive linguistics, the experiential dynamics of metaphor. *Mosaic - A Journal for the Interdisciplinary Study of Literature*, **28**, 1-23.

Haskell, R. E. (Ed.) (1987), *Cognition and Symbolic Structures: The Psychology of Metaphoric Transformation.* Norwood, NJ: Ablex.

Hauser, M. D. (2000), *Wild Minds: What Animals Really Think.* The Penguin Press.

Helman, D. H. (Ed.) (1988), *Analogical Reasoning - Perspectives of Artificial Intelligence, Cognitive Science, and Philosophy.* Kluwer Academic Publishers.

Hess, R. and D. Field (1999), Integration of contours: new insights. *Trends in Cognitive Sciences*, **3**, 480-486.

Hesse, M. B. (1974), *The Structure of Scientific Inferences.* Berkeley: University of California Press.

Hintikka, J. (Ed.) (1994), *Aspects of Metaphor.* Dordrecht: Kluwer Academic Publishers.

Hintikka, J. and G. Sandu (1994), Metaphor and other kinds of nonliteral meaning. In J. Hintikka (Ed.) (1994).

Holyoak, K. J. (1982), An analogical framework for literary interpretation. *Poetics*, **11**, 105-126.

Holyoak, K. J. (1985), The pragmatics of analogical transfer. In G. H. Bower (Ed.), *The Psychology of Learning and Motivation* (vol.19, pp.59-87). New York: Academic Press.

Holyoak, K. J. and P. R. Thagard (1989), A computational model of analogical problem solving. In S. Vosniadou and A. Ortony (Eds.), pp.242-266.

Holzer, P. (1991), Capsaicin: cellular targets, mechanisms of action, and selectivity for thin sensory neurons. *Pharmacological Review*, **43**, 143-201.

Honeck, R. P. (1989), Women, Fire and Dangerous Things. George Lakoff. (Book Review). *Metaphor and Symbolic Activity*, **4**, 279-284.

Hong, S. K., H. C. Han, Y. W. Yoon, and J. M. Chung (1993), Response properties of hypogastric afferent-fibers supplying the uterus in the cat. *Brain Research*, **622**, 215-225.

Hoorn, J. (1997), *Metaphor and the Brain*. Thesis Vrije Universiteit.

Hoorn, J. (1999), Demonstration: RT while processing metaphors. Paper presented at RAAM III conference. Tilburg, The Netherlands.

Humphrey, N. (2000), How to solve the mind-body problem. *Journal of Consciousness Studies*, **7**, 1 - xxx.

Humphreys, G. (1990), Synesthesia: a union of the senses. Richard E. Cytowic. (Book Review). *Nature*, **343**, 30.

Indurkhya, B. (1992), *Metaphor and Cognition: An Interactionist Approach*. Dordrecht: Kluwer Academic Publishers.

Indurkhya, B. (1994), The thesis that all knowledge is metaphorical and meanings of metaphor. *Metaphor and Symbolic Activity*, **9**, 61-73.

Inhelder, B. and J. Piaget (1958), *The Growth of Logical Thinking from Childhood to Adolescence*. New York: Basic Books.

Jackendoff, R. (1992), *Languages of the Mind: Essays on Mental Representation.* Cambridge, Mass.: MIT Press.

Jackendoff, R. (1998), *The Architecture of the Language Faculty*. Cambridge, Mass.: MIT Press.

Jakobson, R. (1968), *Child Language, Aphasia, and Phonological Universals*. Mouton, Paris.

James, I. F., N. N. Ninkina, and J. N. Wood (1993) in *Capsaicin in the Study of Pain* (ed. by J. N. Wood), pp. 83-104. London: Academic Press.

Johnson, C. (1997a), Metaphor Vs conflation in the acquisition of polysemy: The case of SEE. In M. K. Hiraga, C. Sinha and S. Wilcox (Eds.), *Cultural, Topological and Psychological Issues in Cognitive Linguistics*. Current Issues in Cognitive Linguistics Theory 152. Amsterdam: John Benjamins.

Johnson, C. (1997b), Learnability in the acquisition of multiple senses: SOURCE reconsidered. In J. Moxley, J. Juge and M. Juge (Eds.), *Proceedings of the Twenty-Second Meeting of the Berkley Linguistics Society*. Berkley: Berkley Linguistics Society.

Johnson, M. (1987), *The Body in the Mind: The Bodily Basis of Meaning, Imagination, and Reason.* Chicago: University of Chicago Press.

Johnson, M. (1988), Some constraints on embodied analogical reasoning. In: Helman 1988, pp.25-40.

Kagan, J. (1985), The mind as a Necker cube. Open Peer Commentary on J. Fodor's *Modularity of Mind. Behavioral and Brain Sciences*, **8**, 21-22.

Kertesz, A., A. Sheppard and R. MacKenzie (1982), Localization in transcortical sensory aphasia. *Archives of Neurology*, **39**, 475-478.

Kesner, R. P. (2000), Subregional analysis of mnemonic functions of the prefrontal cortex in the rat. *Psychobiology*, **28**, 219-228.

Kilgarriff, A. (1997), "I don't believe in word senses". *Computers and the Humanities*, **31**, 91-113.

Kittay, E. F. (1987). *Metaphor: Its Cognitive Force and Linguistic Structure*. Clarendon Press, Oxford.

Kripke, S. (1972), *Naming and Necessity*. Oxford: Blackwell.

Kuraev, G. A., T. V. Aleinikova, V. N. Dumbaj, and G. L. Feldman (2000), *Fisiologia Centralnoj Nervnoj Sistemy* [Physiology of the Central Nervous System]. Rostov-na-Donu: Fenix.

Lakoff, G. (1987), *Women, Fire, and Dangerous Things: What Categories Reveal about the Mind.* Chicago: The University of Chicago Press.

Lakoff, G. (1993), The contemporary theory of metaphor". In: Ortony, A. 1993, pp. 202-251.

Lakoff, G. and M. Johnson (1980). *Metaphors We Live By*. Chicago: The University of Chicago Press.

Lakoff, G. and M. Johnson (1999), *Philosophy in the Flesh: The Embodied Mind and its Challenge to Western Thought*. New York: Basic Books.

LaMotte, R. H. and J. N. Campbell (1978), Comparison of responses of warm and nociceptive C-fiber afferents in monkey with human judgements of thermal pain. *Journal of Neurophysiology*, **41**,.509-528.

Langacker, R. W. (1988), A usage-based model. In B. Rudzka-Ostyn (Ed.), *Topics in Cognitive Linguistics*. Amsterdam, Philadelphia: John Benjamins.

Larousse Dictionary of Science and Technology (1995), edited by P. M. B. Walker. Larousse plc.

Lass, R. (1997). *Historical Linguistics and Language Change*. Cambridge: Cambridge University Press.

Lehrer, A. and E. F. Kittay (Eds.) (1992), *Frames, Fields, and Contrasts: New Essays in Semantic and Lexical Organization*. Lawrence Erlbaum Associates, Publishers.

Le Robert Quotidien (1996), edited by J. Rey-Debove. Paris: Le Robert.

Lewkowicz, D. J. & G. Turkewitz (1980), Cross-modal equivalence in early infancy: auditory-visual intensity matching. *Developmental Psychology*, **16**, 597-607.

Li, Y.F. (1997), An optimized universal grammar and biological redundancies. *Linguistic Inquiry*, **28**, 170-178.

Liotti, M., H. S. Mayberg, S. K. Brannan, S. McGinnis, P. Jerabek and P. T. Fox (2000), Differential limbic-cortical correlates of sadness and anxiety in healthy subjects: Implications for affective disorders. *Biological Psychiatry*, **48**, 30-42.

Liu, L., A. Szallasi, and S. A. Simon (1998), A non-pungent resiniferatoxin analogue, phorbol 12-phenylacetate 13 acetate 20-homovanillate, reveals vanilloid receptor subtypes on rat trigeminal ganglion neurons. *Neuroscience*, **84**, 569-581.

Longhurst, J.C., M. P. Kaufman, G. A. Ordway, and T. I. Musch (1984), Effects of bradykinin and capsaicin on endings of afferent fibers from abdominal visceral organs. *American Journal of Physiology*, **247**, R552-R559.

Ludlow, P. (1991), Book review of *Metaphor: Its Cognitive Force and Linguistic Structure* by Eva Kittay. *The Journal of Philosophy*, **88**, 324-330.

Luria, A.R. (1966), *Higher Cortical Functions in Man.* New York: Basic Books.

Luria, A. R. (1968), *The Mind of a Mnemonist*. New York: Basic Books.

Lyons, J. (1968), *Introduction to Theoretical Linguistics*. Cambridge: Cambridge University Press.

MacCormac, E. R. (1985), *A Cognitive Theory of Metaphor*. Cambridge: The MIT Press.

MacWhinney, B. (1995), *The CHILDES Project: Tools for Analyzing Talk*. Hillsdale, N. J.: Erlbaum.

Magnie, M.-N., C. T. Ferreira, B. Giusiano and M. Poncet (1999), Category specificity in object agnosia: preservation of sensorimotor experiences related to objects. *Neuropsychologia*, **37**, 67-74.

Mandler, J. M. (1992), How to build a baby II: Conceptual primitives. P*sychological Review,* **99,** 587–604.

Mandler, J. M. and L. McDonough (1998), Studies in inductive inference in infancy. *Cognitive Psychology*, **37**, 60-96.

Maratsos, M. (1989), Metaphors of language: metaphors of the mind? (Book Review of Lakoff 1987). *Contemporary Psychology*, **34**, 5-7.

Mareschal, D. and T. R. Shultz (1999) Development of children's seriation: A connectionist approach. *Connection Science*, 11, 149-186.

Margalit, A. and N. Goldblum (1994), Metaphors in an open-class test. In Hintikka 1994.

Marks, L.E. (1974), On associations of light and sound: The mediation of brightness, pitch and loudness. *American Journal of Psychology*, **87**, 173-188.

Marks, L.E. (1975), On colored-hearing synesthesia: Cross-modal translations of sensory dimensions. *Psychological Bulletin*, **82**, 303-331.

Marks, L.E. (1978), *The Unity of the Senses: Interrelations among the Modalities*. New York: Academic Press.

Marks, L.E. (1982a), Bright sneezes and dark coughs, loud sunlight and soft moonlight. *Journal of Experimental Psychology: Human Perception and Performance*, **8**, 177-193.

Marks, L.E. (1982b), Synesthetic perception and poetic metaphor. *Journal of Experimental Psychology: Human Perception and Performance*, **8**, 15-23.

Marks, L. E., R. J. Hammeal, M. H. Bornstein (1987), *Perceiving Similarity and Comprehending Metaphor*. Child Development Publications, Chicago: The University of Chicago Press.

Marks, L .E. and M. H. Bronstein (1987), Sensory similarities: classes, characteristics, and cognitive consequences". In R. E. Haskell (Ed.), *Cognition and Symbolic Structures: The Psychology of Metaphoric Transformation.* Norwood: NJ:Ablex.

Martin, A., J. V. Haxby, F. M. Lalonde, C. L. Wiggs and L. G. Ungerleider (1995), Discrete cortical regions associated with knowledge of color and knowledge of action. *Science,* **270**, 102-105.

Martin, A., C. L. Wiggs, L. G. Ungerleider and J. V. Haxby (1996), Neural correlates of category-specific knowledge. *Nature,* **379**, 649-652.

Martin, J. and R. Harre (1982), Metaphors in science. In D. Miall (Ed.), *Metaphor: Problems and Perspectives*. The Harvester Press Limited.

Masson, C., P. Koskas, J. Cambier and M. Masson (1991), Left pseudothalamic cortical syndrome and asymbolia for pain. *Revue Neuorologique*, **147**, 668-670.

Matteson, M. A., A. D. Linton, B. L. Cleary, S. J. Barnes and M. J. Lichtenstein (1997), Management of problematic behavioral symptoms associated with dementia: A cognitive developmental approach. *Aging – Clinical and Experimental Research*, **9**, 342-355.

McClelland, J. L., M. St.John and R. Taraban (1989), Sentence comprehension: A parallel distributed processing approach. *Language and Cognitive Processes*, **4**, SI 287-335.

Medin, D. L., R. L. Goldstone and D. Gentner (1993), Respects for similarity. *Psychological Review*, **100**, 254-278.

Melara, R. D. and L. E. Marks (1990), Processes underlying dimensional interactions: correspondences between linguistic and nonlinguistic dimensions. *Memory and Cognition*, **18**, 477-495.

Meltzoff, A. N. and R. W. Borden (1979), Intermodal matching by human neonates. *Nature*, **282**, 403-404.

Mezey, E., Z. E. Toth, D. N. Cortright, M. K. Arzubi, J. E. Krause, R. Elde, A. Guo, P. M. Blumberg, A. Szallasi (2000), Distribution of mRNA for vanilloid receptor subtype 1 (VR1), and VR1-like immunoreactivity, in the central nervous system of the rat and human. *Proceedings of the National Academy of Sciences of the United States of America*, **97**, 3655-3660.

Michalski, R. S. (1989), Two-tired concept meaning, inferential matching, and conceptual cohesiveness. In S. Vosniadou and A. Ortony (Eds.), pp.122-145.

Middle English Dictionary (1952), edited by H. Kurath, S. M. Kuhn, J. Reidy and R. E. Lewis. Ann Arbor: University of Michigan Press.

Miller, G. A. (1979), Images and models, similes and metaphors. In: A. Ortony (Ed.), *Metaphor and Thought*, 357-400.

Miller, G. A. and P. N. Johnson-Laird (1978), *Language and Perception*. Cambridge: Cambridge University Press.

Millikan, R.G. (1984), *Language, Thought and Other Biological Categories: New Foundations for Realism*. Cambridge, MA: MIT Press.

Modern English-Russian Dictionary (1995), edited by V. K. Muller, 2nd edition. Moskva: Russky Yasyk.

Morgan, J. L. (1993). Observations on the pragmatics of metaphor. In A. Ortony (Ed.) (1993).

Murphy, G. L. (1996), On metaphoric representation. *Cognition*, **60**, 173-204.

Murphy, G. L. (1997), Reasons to doubt the present evidence for metaphoric representation. *Cognition*, **62**, 99-108.

Nagy, I. and H. P. Rang (1999), Similarities and differences between the responses of rat sensory neurons to noxious heat and capsaicin. *Journal of Neuroscience*, **19**, 10647-10655.

Narayanan, S. (1997), Talking the talk is like walking the walk: a computational model of verbal aspect. In M. G. Shafto and P. Langley (Eds.), *Proceedings of the Nineteenth Annual Conference of the Cognitive Science Society*. Mahwah, N. J.: Erlbaum.

Ortony, A. (Ed.) (1993/ 1979: first edition). *Metaphor and Thought*. Cambridge: Cambridge University Press.

Osgood, C. E., G. J. Suci, and P. H. Tannenbaum (1957) *The Measurement of Meaning*. Urbana: University of Illinois Press.

Oxford Latin Dictionary (1982), edited by P. G. W. Glare. Oxford: Clarendon Press

Paton, R. G., H. S. Nwans, H.S., M. J. R. Shave and T. J. M. Bench-Capon (1994), An examination of some metaphorical contexts for biologically motivated computing. *British Journal for the Philosophy of Science*, **45**, 505-525.

Peacocke, C. (1992), *A Study of Concepts*. Cambridge MA: MIT Press.

Piaget, J. (1961/1969 English translation), *The Mechanisms of Perception* (trans. G.N. Seagrim). London: Routledge & Kegan Paul.

Piaget, J. (1980a), Introductory remarks with the subsequent discussion. In Piatelli-Palmarini (Ed.), pp. 57-67.

Piaget, J. (1980b), Schemes of action and language learning. With the subsequent discussion. In M. Piatelli-Palmarini (Ed.), pp. 163-183.

Piatelli-Palmarini, M. (Ed.) (1980), *Language and Learning: The Debate between Jean Piaget and Noam Chomsky*. London: Routledge & Kegan Paul.
Pinker, S. (1997), *How the Mind Works*. The Softback Preview.
Plotkin, H. (1997), *Evolution in Mind*. London: Alan Lane.
Pustejovsky, J. (1995), *The Generative Lexicon*. Cambridge, Mass.: MIT Press.
Pustejovsky, J. (1998), Generativity and explanation in semantics: A reply to Fodor and Lepore. *Linguistic Inquiry*, **29**, 289-311.
Pustejovsky, J. and B. Boguraev (Eds.) (1996), *Lexical Semantics: The Problem of Polysemy*. Clarendon Press, Oxford.
Putnam, H. (1975), The meaning of meaning. In his *Mind, Language and Reality*. Cambridge: Cambridge University Press.
Putnam, H. (1980), What is innate and why. In Piatelli-Palmarini 1980, pp.287-309.
Pylyshyn, Z. (1981), The imagery debate: analogue media vs. tacit knowledge. *Psychological Review*, **88**, 16-45.
Pylyshyn, Z. (1999), Review of J. A. Fodor, *Concepts. Trends in Cognitive Sciences*, **3**, 81-82.
Quartz, S. R (1999), The constructivist brain. *Trends in Cognitive Sciences*, **3**, 48-57.
Quine, W. V. (1953), Two dogmas of empiricism. In his *From a Logical Point of View*. Harvard University Press, Cambridge, Mass., pp.20-46.
Reddy, M. J. (1993/1979), The conduit metaphor: A case of frame conflict in our language about language. In A. Ortony (Ed.), pp.164-201.
Regier, T. (1996), *The Human Semantic Potential: Spatial Language and Constrained Connectionism*. Cambridge, MA: MIT Press.
Rey, G. (1994), Concepts. In S. Guttenplan (Ed.), *A Companion to the Philosophy of Mind*. Blackwell: Blackwell Companions to Philosophy, pp. 185-193.
Rousset, F. (2000), How to "see" the mind. Paper presented at the *Braiding the Multiple Threads of Interdisciplinary Research on Metaphor* Workshop at the Japanese Cognitive Science Society's Annual Meeting, Shizuoka Art and Culture University, Hamamatsu, Japan, 30 June – 2 July.
Sann, H. (1998), Chemosensitivity of nociceptive, mechanosensitive afferent nerve fibres in the guinea-pig ureter. *European Journal of Neuroscience*, **10**, 1300-1311.
Sansoni-Harrap Standard Italian and English Dictionary (1970-76), edited by V. Macchi. London: Harrap.
Schiffer, S. (1998). Lessons in Mentalese: Fodor's theory of mind and its problems. *The Times Literary Supplement*, **4969**, 14-15.
Schiltz, K., K. Trocha, B. M. Wieringa, H. M.Enrich, S. Johannes and T. F. Munte (1999), Neurophysiological aspects of synesthetic experience. *Journal of Neuropsychiatry and Clinical Neurosciences*, **11**, 58-65.
Schumacher, M. A., I. Moff, S. P. Sudanagunta and J. D. Levine (2000), Molecular cloning of an N-terminal splice variant of the capsaicin receptor – Loss of N-terminal domain suggests functional divergence among capsaicin receptor subtypes. *Journal of Biological Chemistry*, **275**, 2756-2762.
Searle, J. R. (1979), Metaphor. In A. Ortony 1979, 83-111.
Searle, J. R. (1983) *Intentionality*. Cambridge: Cambridge University Press.
Sellars, W. (1956), Empiricism and the philosophy of mind. *Minnesota Studies in Philosophy*, **1**, 253.
Shen, Y. (1992), Cognitive aspects of metaphor comprehension - An introduction. *Poetics Today*, **13**, 567-574.
Shen, Y. (1997), Cognitive constraints on poetic figures. *Cognitive Linguistics*, **8**, 33-71.
Shultz, T. R., W. C. Schmidt, D. Buckingham and D. Mareschal (1995), Modeling cognitive development with a generative connectionist algorithm. In G. Halford and T. Simon (Eds.), *Developing Cognitive Competence: New Approaches to Process Modelling*. Hillsdale, NJ: Erlbaum.
Sierra, M. and G. E. Berrios (1998), Depersonalization: Neurobiological perspectives. *Biological Psychiatry*, **44**, 898-908.
Smaller English-Latin Dictionary (1961), edited by Sir W. Smith. London: J. Murray.
Smith, L. B. (1987), Commentary. Perceptual relations and perceptual language. In: L. E. Marks, R. J. Hammeal, M. H. Bornstein (1987), *Perceiving Similarity and Comprehending Metaphor*. Child Development Publications, Chicago: The University of Chicago Press.
Sperber, D. and D. Wilson (1986), *Relevance: Communication and Cognition*. Basil Blackwell.
Spillmann, L. (1997), Guest editorial: Colour in a larger perspective: the rebirth of Gestalt psychology. *Perception*, **26**, 1341-1352.

Spiro, R. J., P. J. Feltovich, R. L. Coulson, and D. K. Anderson (1989), Multiple analogies for complex concepts: antidotes for analogy-induced misconception in advanced knowledge acquisition. In: S. Vosniadou and A. Ortony (Eds.), pp.498-531.

Starr, A., H. Pratt, H. Michalewski, J. Patterson, G. Barrett, F. Swire, L. Deecke, D. Cheyne, J. Kristova and G. Lininger (1991), Physiology of short-term verbal memory. *Journal of Neural Transmission - General Section*, **33**, 7-12.

Steen, G. (1994), *Understanding Metaphor in Literature*. London & New York: Longman.

Sternberg, R. J. (1977), *Intelligence, Information Processing, and Analogical Reasoning: The Componential Analysis of Human Abilities*. Hillsdale, N.J.: Erlbaum.

Sternberg, R. J. (1982), Reasoning, problem solving, and intelligence. In: R. J. Sternberg (Ed.), *Handbook of Human Intelligence*. Cambridge: Cambridge University Press.

Stoffregen, T. A. and B. G. Bardy (forthcoming), On specification and the senses. *Behavioral and Brain Sciences*, **24**, xxx-xxx.

Storfer, M. D. (1999), Précis of *Brain Size, Intelligence and Myopia. International Journal of Neuroscience*, **98**, 153-276.

Sutherland, N. S. (1973), Object recognition. In Carterette and Friedman 1973, 157-185.

Sweetser, E. (1990), *From Etymology to Pragmatics: Metaphorical and Cultural Aspects of Semantic Structure*. Cambridge: Cambridge University Press.

Szallasi, A. (1994), The vanilloid (capsaicin) receptor: receptor types and species specificity. *Genetic Pharmacology*, **25**, 223-243.

Szallasi, A. and P. M. Blumberg (1996), Vanilloid receptors: new insights enhance potential as a therapeutic target. *Pain*, **68**, 195-208.

Szolcsany, J. (1993), In: *Capsaicin in the Study of Pain* (ed. J. Wood), pp. 1-26. London: Academic Press.

Tabossi, P., Colombo, L. and Job, R. (1987), Accessing lexical ambiguity: Effects of context and dominance. *Psychological Research*, **49**, 161-172.

The Concise Oxford Dictionary (1995), edited by D. Thompson. Oxford: Clarendon Press.

The Oxford English Dictionary (1933), edited by J. A. H. Murray. London: Oxford University Press.

The Oxford Russian Dictionary (1984), edited by P. Falla, M. Wheeler and B. Unbegaun. Oxford: Oxford University Press.

Torebjork, H. E. and R. G. Hallin (1973), Perceptual changes accompanying controlled preferential blocking of A and C fibre responses in intact human skin nerves. *Experimental Brain Research*, **16**, 321-332.

Traugott, E. C. and B. Heine (1991), Introduction. In Traugott and Heine (Eds.), *Approaches to Grammaticalization*, volume 1, pp.1-14.

Traugott, E. C. and E. König (1991), The semantics-pragmatics of grammaticalization revisited. In Traugott and Heine (Eds.), *Approaches to Grammaticalization*, volume 1, pp. 189-218.

Turner, M. (1988), Categories and analogies. In: Helman 1988, pp.3-24.

Vocabolario della Lingua Italiana (1988), edited by N. Zingarelli. Bologna: Zanichelli.

Vosniadou, S. (1989), Analogical reasoning as a mechanism in knowledge acquisition: A developmental perspective. In S. Vosniadou and A. Ortony (Eds.), pp.413-437.

Vosniadou, S., A. Ortony, R. E. Reynolds and P. T. Wilson (1984), Sources of difficulty in children's comprehension of metaphorical language. *Child Development*, **55**, 1588-1606.

Vosniadou, S. and Ortony, A. (1989), *Similarity and Analogical Reasoning*. Cambridge University Press, Cambridge.

Wagner, S., E. Winner, D. Cicchetti, and H. Gardner (1981), Metaphorical mapping in human infants. *Child Development*, **52**, 728-731.

Warrington, E. K. and T. Shallice (1984), Category specific semantic impairments. *Brain*, **107**, 829-54.

Way, E. C. (1991), *Knowledge Representation and Metaphor*. Intellect: Oxford.

Wellman, H. M. (1990), *The Child's Theory of Mind*. Cambridge, MA: MIT Press.

Werner, H. and B. Kaplan (1963), *Symbol Formation*. New York: Wiley.

Wierzbicka, A. (1996), *Semantics, Primes and Universals*. Oxford: Oxford University Press.

Williams, J. M. (1976), Synaesthetic adjectives: a possible law of semantic change. *Language*, **52**, 461-478.

Williams, J. N. (1992), Processing polysemous words in context: evidence for interrelated meanings. *Journal of Psycholinguistic Research*, **3**, 193-218.

Wilson, W. A. (1965), Intersensory transfer of normal and brain-operated monkeys. *Neuropsychologia*, **3**, 363-370.

Winner, E. and H. Gardner (1977), The comprehension of metaphor in brain-damaged patients. *Brain*, **100**, 717-729.

Young, J. Z. (1987), *Philosophy and the Brain*. Oxford: Oxford University Press.

Younger, B. A. and L. B. Cohen (1985), How infants form categories. In G. H. Bower (Ed.), *The Psychology of Learning and Motivation*, **19**, 211-247. London: Academic Press, Inc.